UNIVERSITA' DEGLI STUDI DI VERONA

DIPARTIMENTO DI

Scienze Economiche

SCUOLA DI DOTTORATO DI

Scienze Giuridiche ed Economiche

DOTTORATO DI RICERCA IN

Economia e Management

CICLO/ANNO
29°/2013

TITOLO DELLA TESI DI DOTTORATO

Essays on Institutions, Natural Resources and Taxation

S.S.D. SECS-P/01

Coordinatore: Prof. Roberto Ricciuti

Firma _____

Tutor: Prof. Roberto Ricciuti

Firma _____

Dottorando: Dott.ssa Tania Masi

Firma _____

Essays on Institutions, Natural Resources and Taxation
Tania Masi
Doctoral Dissertation
Verona, 14th June 2017

Contents

List of Tables

List of figures

Introduction

Since the seminal works of institutional economics literature (Buchanan and Tullock, 1962; North and Thomas, 1973; North, 1981, 1990), the importance of institutions and government policies for economic performance has been well-known. Indeed, political institutions aggregate citizen and group preferences into specific political outcomes, which in turn affect economic outcomes (Persson and Tabellini, 2005).

The relationship between institutions and development has been investigated by several authors. Among them, Acemoglu et al. (2001) suggest that institutions adopted by European settlers in the colonies between the seventeenth and the nineteenth centuries have persisted to the present and have an effect on income per capita. Consequently, there are "substantial economic gains from improving institutions" (Acemoglu et al., 2001: 1395).

Acknowledging the crucial role of institutions for economic development, this thesis comprises three essays that analyse the impact of internal and external factors on institutions, and how the latter influence development outcomes.

The first essay considers how political regimes are affected by civil society organizations (CSOs) and democracy assistance. According to the academic literature, the economic effects of different types of regime are ambiguous: on the one hand, an expansion of political rights may promote economic rights and thereby stimulate growth; on the other, it may be detrimental to growth due to the activity of interest groups. Despite these contrasting results, democracy promotion has become a crucial goal because it is considered a means to achieve international peace and security, as well as economic and social progress and development.

A wealth of studies has analysed which factors increase the probability of the emergence and consolidation of democracy (Lipset, 1959; Przeworski and Limongi, 1997; Rustow, 1970; Barro, 1999; Acemoglu and Robinson, 2006). Among them, civil society seems to be crucial: when citizens are not well organized, the transition to democracy could be delayed indefinitely, whereas when civil society is developed, repression is more difficult and democracy may be preserved.

The effect of aid on institutions and political reforms is hotly disputed (Crawford, 1997; Knack, 2001, 2004; Goldsmith, 2001; Heckelman, 2010). Aid

may stimulate democracy by promoting economic and social development or by funding institutional reforms. However, it can also have a harmful effect since it provides an alternative to tax collection that makes recipient governments less accountable to their citizens (rentier effect).

The first essay contributes to the literature on civil society and democracy aid investigating whether projects implemented by CSOs improve the democracy level of recipient countries. In particular, it evaluates the effectiveness of projects funded by the United Nations Democracy Fund (UNDEF), a trust fund established in 2005 by the United Nations.

An empirical analysis based on the propensity score matching method is carried out for a sample of developing countries. The treatment group includes countries that benefited from projects implemented by CSOs and funded by UNDEF between 2006 and 2011. The findings indicate that the average treatment effect on the treated (ATT) is positive and significant only when UNDEF-funded projects are repeated over time. This suggests that UNDEF should continue to fund civil society and local non-governmental organizations, but it should also implement long-term planning in order to ensure continuity of the intervention.

Aid has often been compared to natural resources in terms of its possible "rentier effect". Indeed, there is substantial evidence that countries highly endowed in exploitable natural resources perform worse than those lacking this asset (Sachs and Warner, 1999; Rodriguez and Sachs, 1999; Gylfason, 2001; Ross, 2001; Caselli and Michaels, 2013). However, the detrimental effect of natural resources is not homogeneous across countries (Robinson et al., 2006).

The second essay evaluates the impact of resource endowment on institutions taking heterogeneity into account. Specifically, the synthetic control method is used to compare the evolution of the democracy level in countries in which giant oil reserves were discovered with the weighted democracy level of countries that do not undergo the same event and had similar pre-event characteristics.

Focusing on 12 countries in which the rate of oil discoveries started to decline in the 1970s or later (peak of oil discoveries), the variation in oil endowment has a negative effect in the long run in most cases, but countries with a high level of democracy in the pre-event period were unaffected.

Arguing that the effect of natural resources on development outcomes may depend on the type of resources (Isham et al., 2005) and on the quality of the institutional environment (Mehlum et al., 2006; Robinson et al., 2006), the third essay analyses how resource abundance affects fiscal capacity, defined as the amount of taxes that a government could potentially raise given the structure of the tax system and its powers of enforcement. This outcome is strategically important for economic development since the capacity to collect revenue is indispensable to public goods provision and investment in infrastructure (Besley and Persson 2011).

Using panel methods, the essay tests two hypotheses: a) resource rents reduce the incentives to invest in fiscal capacity, thereby resource-rich countries have less developed tax systems and collect a lower share of income taxes in total taxes; b) political institutions placing limits on executive powers promote common interests and, thus, raise the incentives for investing in fiscal capacity. A high level of executive constraints may therefore mitigate or neutralise the negative effect of natural resources on fiscal infrastructures.

Empirical analysis demonstrates that resource rents are negatively associated with fiscal capacity, measured as the share of non-resource taxes on income, profits and capital gains in total non-resource taxes. However, countries with a high level of executive constraints are able to neutralise or even reverse this effect, depending on the type of resource endowments. The paper also provides insight into the specific channels through which natural resources impact on tax systems, suggesting that they affect fiscal institutions that make the state accountable and transparent to its citizens.

References

Acemoglu, D., Johnson, S., Robinson, J. A. (2001). The Colonial Origins of Comparative Development: An empirical Investigation. *American Economic Review*, 91(5), 1369-1401.

Acemoglu, D., Robinson, J. A. (2006). Economic Origins of Dictatorship and Democracy. Cambridge: Cambridge University Press.

Barro, R. J. (1999). Determinants of Democracy. *Journal of Political Economy*, 107(S6), S158-S183.

Besley, T., Persson, T. (2011). *Pillars of prosperity: The political economics of development clusters*. Princeton University Press.

Buchanan, J. M., Tullock, G. (1962). *The Calculus of Consent (Vol. 3)*. Ann Arbor: University of Michigan Press.

Caselli, F., Michaels, G. (2013). 'Do oil windfalls improve living standards? Evidence from Brazil'. *American Economic Journal: Applied Economics*, 5(1): 208-238.

Crawford, G. (1997). Foreign Aid and Political Conditionality: Issues of Effectiveness and Consistency. *Democratization*, 4(3), 69-108.

Gylfason, T. (2001). 'Natural Resources, education, and economic development'. *European Economic Review*, 45: 847-859.

Goldsmith, A. A. (2001). Foreign Aid and Statehood in Africa. *International organization*, 55(01), 123-148.

Heckelman, J. C. (2010). Aid and democratization in the transition economies. *Kyklos*, 63(4), 558-579.

Isham, J., Woolcock, M., Pritchett, L., Busby, G. (2005). The varieties of resource experience: natural resource export structures and the political economy of economic growth. *The World Bank Economic Review*, 19(2): 141-174.

Knack, S. (2001). Aid Dependence and the Quality of Governance: Cross-country Empirical Tests. *Southern Economic Journal*, 68(2), 310-329.

Knack, S. (2004). Does Foreign Aid Promote Democracy? *International Studies Quarterly*, 48(1), 251-266.

Lipset, S. M. (1959). Some Social Requisites of Democracy: Economic Development and Political Legitimacy. *American Political Science Review*, 53(01), 69-105.

Mehlum, H., Moene, K., Torvik, R. (2006). Institutions and the resource curse. *The economic journal*, 116(508): 1-20.

North, D. C. (1981). Structure and Change in Economic History. New York: Norton and Co.

North, D. C. (1990). *Institutions, Institutional Change and Economic Performance*. Cambridge: Cambridge University Press.

North, D. C., Thomas, R. P. (1973). *The Rise of the Western World: A New Economic History*. Cambridge: Cambridge University Press.

Persson, T., Tabellini, G. (2005). *The Economic Effects of Constitutions*. Cambridge: MIT Press.

Przeworski, A., Limongi, F. (1997). Modernization: Theories and Facts. *World Politics*, 49(02), 155-183.

Robinson, J., R. Torvik, and T. Verdier (2006). 'Political Foundations of the Resource Curse'. *Journal of Development Economics*, 79(2): 447–68.

Rodriguez, F., Sachs, J. D. (1999). 'Why do resource-abundant economies grow more slowly?'. *Journal of Economic Growth*, 4(3): 277-303.

Ross, M. L. (2001). 'Does oil hinder democracy?'. *World politics*, 53(3): 325-361.

Rustow, D. A. (1970). Transitions to Democracy: Toward a Dynamic Model. *Comparative Politics*, 2(3), 337-363.

Sachs, J. D., Warner, A. M. (1999). 'The big push, natural resource booms and growth'. *Journal of development economics*, 59(1): 43-76.

1. Strengthening the voice of civil society: The impact of the United Nations Democracy Fund

Abstract

Democracy assistance has become one of the main components of foreign aid programmes. Following this trend, in 2005, the United Nations established the UN Democracy Fund (UNDEF), whose objective is to support projects submitted by NGOs and civil society aimed at increasing government accountability. This paper investigates the impact of civil society organizations on democracy, utilizing the UNDEF database. Empirical analysis based on a propensity score matching method is carried out on a sample of 102 developing countries. In particular, a logistic model is used to match countries that benefited from projects implemented by NGOs and civil society, funded by UNDEF between 2006 and 2011 (treated), with a well-selected control group. The findings indicate that the average treatment effect on the treated (ATT) is positive and significant only when countries implement UNDEF-funded projects for three rounds or more. In this case, for treated countries, the Polity IV indicator improves by an average of 1.28 points compared to the level in 2005.

Keywords: Democracy, United Nations, civil society, propensity score analysis, impact evaluation.

JEL Codes: C31, F53, L31, P48

This essay has benefited from the comments of participants at the 15th Jan Tinbergen European Peace Science Conference, University of Warwick, and the 10th Annual Conference on the Political Economy of International Organizations (PEIO), University of Bern. An excerpt was published in *Peace Economics, Peace Science and Public Policy*, 2015, 21(4), 489-496.

1.1 Introduction

One of the most demanding tasks in development economics is to identify tools and policies that can foster growth and social progress, as well as international peace and security. Foreign aid from developed to developing countries has attracted interest as a potential way to accomplish these goals. The first development assistance programmes were targeted at promoting better economic performance by encouraging investments in agricultural reforms, infrastructure, education and health. Starting from the early 1990s, democracy promotion has become a crucial component of foreign aid. It aims to empower voters and supports political parties, labour unions and advocacy networks.

Civil society has been acknowledged as essential for both democratisation and the maintenance of democracy. This explains why donor countries allocate most democracy aid through civil society organisations (CSOs). According to the World Bank CSOs include community groups, non-governmental organizations (NGOs), labour unions, indigenous groups, charitable organizations, faith-based organizations, professional associations, and foundations.[1] Among them, NGOs became the main actors during the twentieth century. Indeed, the number of NGOs with consultative status with the United Nations Economic and Social Council (ESOCOC) rose from 40 in 1940 to over 3,900 in 2014. More than 38,000 international NGOs are currently working worldwide (Year Book of International Organizations, 2014), and, in 2011, $19.3 billion of official development assistance (ODA) was allocated to and through civil society (OECD, 2013).

Following this trend, the Member States of the United Nations (UN) have committed themselves to sparing no effort in promoting democracy, strengthening the rule of law, and protecting human rights (United Nations, 2000). As a consequence, in 2005, the UN established the United Nations Democracy Fund (UNDEF), whose objective is to support projects implemented by NGOs and civil society in the field of strengthening democratic dialogue, civil society empowerment, civic education, freedom of information, and the rule of law.

This paper intends to contribute to the debate on aid and democratisation by utilizing the UNDEF project database to provide some empirical results on the

[1] http://crinfo.worldbank.org/crinfo/social_responsibility/civil_society.html.

effects of CSO activity on democracy. While much of the literature relies on qualitative case studies, this research conducts a cross-country analysis to evaluate whether and to what extent UNDEF-funded projects are effective in improving the level of democracy of recipient countries. The hypothesis is that countries benefiting from UNDEF-funded CSO projects have higher democracy scores due to the positive effect on grassroots participation and government accountability. However, a threshold number of UNDEF funding rounds may be necessary to achieve the objective.

As highlighted by Acemoglu in his *Boston Review* piece on effective altruism, "a precise measurement of the social value of a donated dollar may be impossible".[2] However, the policy implications of such an evaluation are of great importance and an approach exists that can provide an assessment of the global effectiveness of CSO projects: the propensity score matching (PSM) method. Although generally used in microeconomic applications, PSM overcomes the selection bias problem as well as other specific issues that arise in a macroeconomic context (Persson and Tabellini, 2005).

This paper implements PSM on a sample of 102 developing countries. Empirical analysis shows that benefiting from CSO projects for at least three rounds of UNDEF funding actually increases the level of democracy in recipient countries. In particular, for treated countries, UNDEF projects have higher Polity IV scores by 1.28 compared to 2005. However, the effect is not significant for countries in which CSO projects take place for only one or two rounds. This suggests that UNDEF projects should be repeated over time in order to be effective.

This paper is organized as follows: Section 2 reviews the literature on the role of civil society and foreign aid in democratisation and democratic transitions; Section 3 is dedicated to UNDEF; Section 4 introduces the identification strategy, and the data are described in Section 5; Section 6 presents the results, the robustness of which is checked in Section 7; Section 8 sums up.

[2] http://bostonreview.net/forum/logic-effective-altruism/daron-acemoglu-response-effective-altruism (1st July 2015)

1.2 Civil society, democratisation and democracy aid

This paper refers to two strands in the literature. The first concerns the relationship between civil society, democratisation and democratic consolidation (Diamond, 1994, 1997; Burnell and Calvert, 2004). As recognised by Acemoglu and Robinson (2006), civil society is often considered "the hero of democratic resistance and transition" (Linz and Stepan, 1997: 18). However, few studies properly analyse the interaction between institutions and civil society and non-governmental organizations[3]. In fact, much of this strand of the literature has been produced by NGO activists or social scientists with close links to funding agencies. As a result, it is not clear whether and exactly how civil society organizations contribute to democratisation and to the formal political process (Edwards and Hulme, 1994; Clarke, 1998).

Hirschman (1987) argues that it is impossible to prove a connection between the decline of the authoritarian state in Latin America and the rise of NGOs and grassroots social movements. Bratton (1989) maintains that African governments have responded ambiguously to the appearance of NGOs. On the one hand, they have valued the economic resources that NGOs can provide. On the other, they have resisted the political pluralisation involved in popular development actions. Thus, in the '80s, African governments were neither as democratically responsive as their South Asian counterparts, nor as effective at authoritarian control as Latin American military governments.

Sanyal (1994) discusses the bottom-up development efforts led by NGOs. He claims that the political impact of bottom-up projects has been even less evident than their economic impact. The lack of political impact of these projects has two causes: a) implementation often requires the support of the local elite; b) the NGOs that implement the bottom-up projects usually lack institutional linkage with political parties and the government. Conversely, Clarke (1998) reports significant contributions of NGOs to democratisation and political change. In particular, they helped restore democracy in Chile in 1990, and in the Philippines in 1992.

[3] See Mercer (2002) for a critical review of the literature on the relationship between NGOs, civil society and the state.

Rahman (2006) links the erosion of democratic institutions to the de-politicisation of NGOs. Examining the case of Bangladesh, the author claims that the NGO sector has shifted away from its initial focus on promoting political mobilisation and empowerment, and has become a provider of goods. This change has led to a macro-level crisis in democratic institutions and the public sphere in Bangladesh.

The second strand of the literature to which this paper refers is the impact of foreign aid on democracy. Here too, academic research has come to a variety of conclusions. Crawford (1997) and Knack (2004) show that there is no evidence that aid promotes political reform or democracy, whereas aid dependence can actually erode the quality of government (Knack, 2001). On the contrary, Goldsmith (2001) finds a positive relationship between Official Development Assistance (ODA) and the level of democracy in Africa, and Heckelman (2010) concludes that aid has been beneficial to democratic reforms in the transition economies of Eastern Europe and former Soviet Union.

Finkel *et al.* (2007) claim that researchers should not lump together democracy assistance with programmes designed to improve health, education, the environment, or economic growth.[4] Based on this distinction, the authors analyse the effect of the U.S Agency for International Development (USAID) on the level of democracy in 165 countries between 1990 and 2003. Their findings show that an investment of one million dollars fosters an increase in democracy which is 65 percent more than the change expected for the average country in the sample, in any given year. Scott and Steele (2011) reach similar conclusions examining the impact of democracy aid from USAID on democratisation in Latin America, the Middle East, Africa and Asia between 1988 and 2001. Using a simultaneous equation model, they confirm the positive effect of democracy aid, while general foreign economic aid does not have a significant impact on democratisation.

Finally, a few papers analyse the link between aid programmes and civil society organisations. Howell (2000) looks at donor attempts to strengthen civil

[4] Democracy assistance is defined as "aid that is specifically designed to foster a democratic opening in a nondemocratic country or to further democratic transition in a country that has experienced a democratic opening" (Carothers, 1999: 6).

society from the outside. The author shows that donors have played a significant role in shaping civil society in many aid-recipient countries by supporting some organizations and excluding others. Specifically, after the cold war, donors focused on urban and formal organisations engaged with the state at the national level. This ignores the complex effects of class, ethnicity and gender in political processes. Examining the role assigned to civil society in South Africa, Hearn (2000) shows that donors have favoured CSOs concerned with promoting the values, procedures and overall framework of democracy, rather than advocacy NGOs, which tackle a single issue. After the election of 1994, these organizations have played a crucial role in connecting new government structures with South African society as a whole. However, given the focus of aid programmes on political stability rather than on socioeconomic transformation, they have failed to reduce inequality. Savun and Tirone (2011) claim that democracy aid not only helps democratic transitions, but also decreases the risk of conflict by increasing the accountability of incumbents and by empowering civil society organizations. In particular, CSOs limit state power and subject government actions to close public scrutiny.

1.3 The United Nations Democracy Fund

The United Nations Democracy Fund (UNDEF) was established by the UN Secretary-General in July 2005 as a UN General Trust Fund.[5] The purpose of UNDEF is to support democratisation by funding projects that strengthen the voice of civil society, promote human rights, and encourage the participation of all in the democratic process. Projects are for two years and include the strengthening of democratic dialogue, civil society empowerment, civic education, freedom of information, and the rule of law.

UNDEF is funded entirely by voluntary contributions from governments. Since 2005, it has received more than 150 million dollars from forty countries, including developing countries. Table 1.1 shows contributions.

[5] See UN General Assembly resolution A/RES/60/1 (paragraphs 135-137, page 30). For a discussion of the definition of trust funds, see Macy (1972).

[Table 1.1 about here]

Projects may be implemented by governments, national and intergovernmental bodies, regional units and UN entities. However, UNDEF favours projects implemented by civil society and non-governmental organizations. Indeed, of the 405 local projects carried out from 2006 to 2012, 365 were implemented by civil society and NGOs.[6] These organizations completed 117 projects in Sub-Saharan Africa, 93 in Asia and the Pacific, 55 in the Americas, and 50 in both Europe and the Arab States.

In most cases, no more than one project is carried out in a given country in any one year. Few countries are involved in two projects per year. Three projects took place in Iraq in 2006. UNDEF grants range from $50,000 to $500,000, and the average amount approved is over $260,000. Table 1.A.1 in the Appendix lists the amount and the number of projects by country and year. Table 1.2 provides some descriptive statistics.

[Table 1.2 about here]

UNDEF does not seek projects, but exerts some influence at the design stage. Since UNDEF acts as a project taker rather than a project maker, it cannot identify and address specific needs. Nevertheless, UNDEF projects do meet real needs thanks to its selection process (MacKellar *et al.*, 2014). Generally, projects that focus on tightly defined target beneficiary groups tend to have higher scores for effectiveness and relevance. Grantees are selected by a rigorous and competitive process that includes quality control and due diligence.

The UN Secretary-General-appointed Advisory Board evaluates funding proposals. It represents or includes: a) the seven largest Member State contributors, b) the Executive Director of the United Nations Office for Partnership (UNOP), c) six other Member States from different regions, d) three individual members, and

[6] See the UNDEF project database available at *http://www.un.org/democracyfund/searchform.*

e) two representatives of civil organizations. The Advisory Board recommends funding proposals for approval by the Secretary-General.[7]

All projects are assessed after completion. Transtec, the UNDEF-contracted commercial evaluator, has assessed most of them. Transtec ensures the independent and transparent assessment of UNDEF projects. It combines qualitative and quantitative approaches and provides statistical and numerical evidence of performance, as well as informed opinions and the experience of key partners, stakeholders and beneficiaries, providing a better understanding of the effects of the interventions and analysing the lessons learnt.[8] However, no evaluation of the average effect of UNDEF projects on political institutions has been carried out.

Although the amount approved for each project is not high, the selection process and the evaluation after completion guarantee the effectiveness of civil society and NGO activity. This means that countries benefiting from UNDEF-funded CSO projects should show a higher level of grassroots participation and government accountability, which, in turn, should have a positive effect on the country's democracy score. The following sections test this hypothesis.

1.4 The identification strategy

The identification of the impact of civil society and NGO projects on democracy runs into the "fundamental problem of causal inference" (Holland, 1986: 947). This problem arises when, for a given unit exposed to a programme or treatment, the alternative state of affairs in the absence of the intervention is unobservable, and therefore the effect of treatment is unidentifiable. The *Neyman-Rubin counterfactual framework of causality* (Neyman, 1935; Rubin, 1974) overcomes this problem by evaluating the mean outcome of treatment participants and the mean outcome of non-treatment participants among the population.[9] According to

[7] For details on UNDEF governance, see the Terms of Reference available at *http://www.un.org/democracyfund/terms-reference*.

[8] See *http://www.transtec.be*

[9] Let $E(Y_1 | W = 1)$ denote the mean outcome of the individuals who comprise the treatment group, and $E(Y_0 | W = 0)$ denote the mean outcome of the individuals who comprise the control group. The average treatment effect (ATE) is defined as $\tau = E(Y_1 | W = 1) - E(Y_0 | W = 0)$.

this framework, the standard estimator of the treatment effect is consistent if the mean outcome for the treated group under the status of non-treatment is the same as the mean outcome of the control group, and the mean outcome for the latter under the status of treatment is the same as the mean outcome of the former.[10]

This condition is met in randomized experiments, in which treatments are allocated randomly to experimental units (Fisher, 1935; Kempthorne, 1952; Cox, 1958). By contrast, observational studies, including the empirical analysis undertaken in this paper, lack random assignment and are subject to selection bias. Indeed, CSOs choose to submit their project proposals (*self-selection*) and are selected by UNDEF (*administrator selection*). To solve this problem, Propensity Score Matching (PSM) can be implemented (Rosenbaum, 2002; Rosenbaum and Rubin, 1983). The matching approach compares differences in outcomes between the treatment participants and a well-selected control group. This group comprises those individuals that do not participate in the intervention and are similar to the participants in all relevant pre-treatment characteristics X. PSM develops a single score that captures all the relevant characteristics, rather than requiring a one-to-one match of each $\boldsymbol{x}$. The propensity score is defined as the conditional probability of receiving treatment ($W = 1$) given a vector of observed characteristics (covariates $\boldsymbol{X}$):

$$p(\boldsymbol{X}) = P(W = 1|\boldsymbol{X}) \qquad [1.1]$$

Conditional on the propensity score, potential outcomes are independent of treatment assignment, as in randomized experiments (the *unconfoundedness assumption*). If this assumption holds, and units with the same $\boldsymbol{x}$ values have a positive probability of being both participants and non-participants (*overlap assumption*), then the mean difference of the outcome variable between treated and control participants for all units with the same value of propensity score is an unbiased estimate of the average treatment effect (ATE):

[10] $E(Y_0|\,W = 1) = E(Y_0|\,W = 0)$ and $E(Y_1|\,W = 0) = E(Y_1|\,W = 1)$, where $E(Y_0|\,W = 1)$ and $E(Y_1|\,W = 0)$ are potential outcomes.

$$\tau = E[E(Y_1|p(\boldsymbol{X}), W_i = 1) - E(Y_0|p(\boldsymbol{X}), W_i = 0)] \quad [1.2]$$

Focusing on the effects on those who actually participated in the treatment, it is possible to define the average treatment effect on the treated (ATT) as the difference between the expected outcome values with and without treatment for the participants:[11]

$$\tau_{ATT} = E_{p(\boldsymbol{X})|W=1}[E(Y_1|p(\boldsymbol{X}), W_i = 1) - E(Y_0|p(\boldsymbol{X}), W_i = 0)] \quad [1.3]$$

The propensity scores are estimated using logistic regression, i.e. the conditional probability of receiving the treatment is defined as follows:[12]

$$P(W_i = 1|\boldsymbol{X}_i) = E(W_i) = \frac{e^{x_i\beta_i}}{1 + e^{x_i\beta_i}} = \frac{1}{1 + e^{-x_i\beta_i}} \quad [1.4]$$

The selected matching algorithm is *kernel matching* (KM). With respect to other algorithms, KM provides a lower variance of the estimator because more information is used.[13] Indeed, this is a non-parametric matching estimator using the weighted averages of all individuals in the control group to construct the counterfactual outcome. Weights are inversely proportional to the distance between propensity scores for treated participants and controls. The KM estimator of the ATT is given by:

[1.5]

[11] Heckman (1997) claims that ATE is not policy relevant because interest should focus on the effects of programmes on intended recipients, and exclude persons for whom the programme was never intended.

[12] For details on the estimation of the propensity scores, see Guo and Fraser (2015).

[13] Caliendo and Kopeinig (2008) discuss the trade-offs in terms of bias and efficiency of the matching algorithms.

$$\tau_{ATT}^{K} = \frac{1}{N_{W=1}} \sum_{i\in(W=1)} \left\{ Y_{i(W=1)} - \frac{\sum_{j\in(W=0)} Y_{j(W=0)} K\left(\frac{p(\boldsymbol{X})_j - p(\boldsymbol{X})_i}{h_n}\right)}{\sum_{k\in(W=0)} K\left(\frac{p(\boldsymbol{X})_k - p(\boldsymbol{X})_i}{h_n}\right)} \right\}$$

where i is a treated unit, j is a control unit, $Y_{i(W=1)}$ and $Y_{j(W=0)}$ are the observed outcomes of the treated and control units respectively, $N_{W=1}$ is the number of units in the treated group, h_n is a bandwidth parameter and K(·) is the kernel function. In this paper, the counterfactual outcome of Y_{0i} is estimated using fixed bandwidth and the *Epanechnikov kernel*:[14]

$$K(u) = \frac{3}{4}(1-u^2)1_{\{|u|\leq 1\}}. \qquad [1.6]$$

1.5 Data

Coming to the data, the level of democracy of recipient countries is evaluated via the Polity IV Project (Marshall *et al.*, 2014), which provides a 21-point scale ranging from -10 (hereditary monarchy) to +10 (consolidated democracy). The Polity IV score is a composite indicator derived from the weighted average of the following components: a) competitiveness of political participation, b) regulation of participation, c) openness and competitiveness of executive recruitment, d) constraints on the chief executive.

The propensity score is estimated with a set of covariates related to both the level of democracy and the presence of CSOs in the countries, but unaffected by participation in the programme. Firstly, real GDP per capita is used to take into account the broadly acknowledged correlation between democracy and this measure of economic performance. Secondly, the amount of Official Development Assistance (ODA) is used as a proxy for the presence of the organizations in the

[14] The estimator of the counterfactual outcome of Y_{0i} is given by $\frac{\sum_{j\in(W=0)} Y_{j(W=0)} K\left(\frac{p(\boldsymbol{X})_j - p(\boldsymbol{X})_i}{h_n}\right)}{\sum_{k\in(W=0)} K\left(\frac{p(\boldsymbol{X})_k - p(\boldsymbol{X})_i}{h_n}\right)}$.

For details on the Epanechnikov kernel, see Epanechnikov (1969).

country.[15] Thirdly, the urban population as a percentage of total population is taken to be related to both democracy and CSO activity.[16] The data for these three variables come from the United Nations Conference on Trade and Development (UNCTAD) database.[17] Finally, the Freedom House database is used to consider one of the main fields of UNDEF projects: freedom of information.[18] More precisely, Freedom House provides a score for press freedom on a 100-point scale. In this paper, the lower the numeric score, the lower the press freedom, and the opposite for the Freedom of the Press Index. Table 1.3 sets out the data description and sources.

[Table 1.3 about here]

Logistic regression uses the mean value of each covariate computed in the period from 2000 to 2005. This ensures that neither temporary shocks nor participation in the projects affects the probability of being in the treated group. Furthermore, to remove any bias from unobservable characteristics, the outcome variable (democracy) is given by the difference between the Polity scores in 2012 and 2005. These data are available for a sample of 102 developing countries. Table 1.4 provides summary statistics of the outcome variable and covariates.

[Table 1.4 about here]

Figures 1.1 and 1.2 map the distribution of types of regime in 2005 and in 2012 respectively. Polity scores are converted into regime categories as suggested by the Polity IV Project: autocracy from -10 to -6 of the Polity IV scale, closed

[15] For details of the increasing role of NGOs in managing and delivering ODA, see OECD (2013).

[16] The literature shows mixed but significant results for the relationship between democracy and urbanization. Moreover, given their purpose, NGO projects approved by UNDEF are more likely to be developed in urban areas.

[17] The database is available at https://unctadstat.unctad.org./

[18] The database is available at https://freedomhouse.org/

anocracy[19] from -5 to 0, open anocracy from 1 to +5, democracy from +6 to +9, and full democracy +10.

[Figure 1.1 about here]

[Figure 1.2 about here]

Of the 102 countries in the sample, from 2006 to 2011, eighteen were not involved in UNDEF projects, 18 benefited for one round only, 22 for 2 rounds, 17 for 3, 11 for 4, 12 for 5, and 4 countries for 6 rounds. The next section presents the empirical results considering in the treatment group, firstly countries in which CSO projects took place for at least a round, then countries benefiting for at least two rounds, and, finally, those involved for at least three rounds.

1.6 Results

Figures 1.3 to 1.5 map the distribution of treated and control countries placing in the treatment group all countries in which CSO projects took place for at least one round, two rounds, and three rounds respectively.

[Figure 1.3 about here]

[Figure 1.4 about here]

[Figure 1.5 about here]

When the treatment group includes countries that benefited for at least one or two rounds, four treated countries are discarded in the matching procedure since they are off support. Table 1.5 provides the estimate of the average treatment effect on the treated (ATT). In the first two specifications, the ATT is positive but not

[19] Marshall and Cole (2014: 21) define anocracies as "countries whose governments are neither fully democratic nor fully autocratic but, rather, combine an often incoherent mix of democratic and autocratic traits and practices."

significant. This means that, on average, CSO projects do not affect the level of democracy in recipient countries.

[Table 1.5 about here]

Considering at least three rounds, 44 countries belong to the treated group and 58 to the control group.[20] Figure 1.6 shows that the common support is wide and all the treated units are on support in this case. Thus, a correct causal inference can be made for the entire treated group.

[Figure 1.6 about here]

Figure 1.7 provides the boxplot of the estimated propensity scores grouped for treated and control countries, and confirms this inference. The treated boxplot shows that the median of the propensity score for this group is around 0.5, whereas the control group has a median below 0.4. Moreover, while the treated countries are distributed symmetrically, the distribution of control units is slightly right-skewed. Nevertheless, neither of the two groups includes any outlier. This condition and the existing overlap between the distributions of the two groups show how treated and control countries can be matched effectively. The boxplot of the estimated propensity score for the matched sample, given in Figure 1.8, proves that propensity score kernel matching does generate a control group similar enough to the treated group. In this figure, most observations of treatment and control groups have the same likelihood of benefiting from UNDEF projects.

[Figure 1.7 about here]

[Figure 1.8 about here]

The covariate imbalance before and after matching confirms the comparability of the two groups. Indeed, Table 1.6 shows that the reduction in bias

[20] They are listed in Table 2.A.2 in the Appendix.

is over 70% for all covariates except for the amount of ODA per capita, for which it is around 30%. Nevertheless, the p-value of the t-test suggests that the mean value of each variable is the same in the treated and control group. Figure 1.A.1 in the Appendix gives the boxplots of the covariate imbalance between treated and control countries for each variable.

[Table 1.6 about here]

Having verified the *overlap assumption* and assuming that the *unconfoundedness assumption* holds, the last row of Table 1.5 provides an unbiased estimate of the average treatment effect on countries benefiting from CSO projects for at least three UNDEF rounds. In this case, the ATT is positive and significant at the 0.05 level. This means that CSO projects taking place for at least three rounds of UNDEF funding increase the level of democracy in recipient countries. More precisely UNDEF projects have raised the Polity IV score by 1.28 compared to the level in 2005.

1.7 Robustness checks

The implementation of propensity score matching entails a number of decisions concerning the estimation of the propensity score, which may affect the results. This section assesses the sensitivity of the estimated ATT to different specifications of the PSM.

The first choice refers to the estimation model. Table 1.7 presents the estimates of the ATT using a probit rather than logit model. Continuing to impose the common support restriction, this model means that when the treatment group includes countries that benefited for at least one round, five treated countries are discarded since they are not on support. Four countries are off support considering at least two rounds. Nevertheless, the sign and significance of the parameters do not differ from the previous estimate.

[Table 1.7 about here]

The second choice concerns the matching algorithm. *Nearest neighbour* (NN) *matching* is the most straightforward estimator. Caliendo and Kopeinig (2008) suggest using more than one NN. This allows variance to be reduced by using more information to construct the counterfactual, but it also increases the bias. Table 1.8 shows the results obtained considering five, ten and twenty matching partners. In this case too, the effect of CSO projects is significant only when the treated group includes countries benefiting from at least three UNDEF-funding rounds. However, as the number of NNs increases, the ATT decreases. This confirms that projects should be repeated over time, but it also suggests that the effect of CSO projects may be even higher than the one presented in the previous section.

[Table 1.8 about here]

Implementing *kernel matching*, two decisions have to be made, the first about the kernel function and the second about the bandwidth parameter. The former is assessed using a normal and a tricube function instead of the *Epanechnikov kernel*. Table 1.9 shows that it does not affect the results; the ATT is only slightly smaller considering the normal function.

[Table 1.9 about here]

The choice of the bandwidth parameter involves a trade-off between the bias and variance of the density function. On the one hand, the higher the bandwidth parameter the smoother the estimated density function, and therefore the smaller the variance between the estimated and the true underlying density function. On the other hand, a smaller parameter reduces the bias, but increases the variance. The estimates of the ATT presented in Table 1.5 are obtained using a fixed bandwidth parameter of 0.06, and Table 1.10 sets out the results of the matching procedure considering bandwidths of 0.04 and 0.08. These specifications confirm previous results in terms of the size of the ATT, but a bandwidth value of 0.04 leads to a significant effect, including for countries involved for at least two rounds. However,

in this case, the effect is significant at the 0.10 level, whereas the significance level remains 0.05 considering at least three rounds.

[Table 1.10 about here]

1.8 Conclusions

This is the first paper that empirically tests the hypothesis that benefiting from CSO projects granted by UNDEF has a positive effect on the level of democracy in recipient countries. The UNDEF project database provides the conditions for evaluating the treatment effect and enables cross-country analysis to be carried out, rather than a qualitative case study.

The analysis shows that, for countries benefiting from at least three rounds of UNDEF funding, CSO projects raised the Polity IV score by 1.28 compared to the level in 2005. In contrast, the effect is not significant for countries involved for fewer years. Robustness checks support these results and suggest that the effect may be even higher.

The findings indicate that specific projects may be more effective for democratisation than large amounts of general economic aid (Scott and Steele, 2011), and are consistent with Persson and Tabellini (2009) who claim that democracy emerges through the slow accumulation of civic and social assets. As a policy consequence, this suggests that UNDEF should continue to fund civil society and local NGOs, but should implement long-term planning and support projects over time. This may be particularly positive for African countries, most of which are anocracies with a chance to move towards democracy. Moreover, since the institutional effects unfold over time and are cumulative (Gerring *et al*., 2005), it could assist developing countries in reaching the path to economic growth.

CSO projects may have a beneficial effect on the quality of the institutions in the recipient country through two mechanisms that are mutually reinforcing. On the one hand, ex-post evaluation indirectly affects not only the CSO involved in the project, but also the institutions benefiting from its activity. Consequently, the accountability of local governments increases. On the other hand, when interaction

between UNDEF governance and the recipient country is repeated over time, CSO projects are more likely to be effective. In either case, the plausible positive effect on grassroots participation and government accountability should spill over and affect the components of the Polity IV score. Further studies should investigate these mechanisms.

References

Acemoglu, D., Robinson, J. A. (2006). *Economic Origins of Dictatorship and Democracy*. Cambridge: Cambridge University Press.

Bratton, M. (1989). The Politics of Government-NGO Relations in Africa. *World Development, 17(4), 569-587.*

Burnell, P. J., Calvert, P. (Eds.). (2004). *Civil society in Democratization*. London: Frank Cass.

Caliendo M., Kopeinig, S. (2008). Some Practical Guidance for the Implementation of Propensity Score Matching. *Journal of Economic Surveys, 22(1), 31-72.*

Carothers, T. (1999). *Aiding Democracy Abroad: The Learning Curve.* Washington: Carnegie Endowment for International Peace.

Clarke, G. (1998). Non-governmental Organizations (NGOs) and Politics in the Developing World. *Political Studies, 46(1), 36-52.*

Cox, D. R. (1958). *Planning of Experiments*. New York: John Wiley & Sons.

Crawford, G. (1997). Foreign Aid and Political Conditionality: Issues of Effectiveness and Consistency. *Democratization, 4(3), 69-108.*

Diamond, L. J. (1994). Toward Democratic Consolidation. *Journal of Democracy, 5(3), 4-17.*

Diamond, L. J. (Ed.). (1997). *Consolidating the Third Wave Democracies: Themes and Perspectives*, Baltimore: Johns Hopkins University Press.

Edwards, M., Hulme, D. (1994). NGOs and Development: Performance and Accountability in the "New World Order". *International Workshop on NGOs and Development*, University of Manchester.

Epanechnikov, V. A. (1969). Non-parametric Estimation of a Multivariate Probability Density. *Theory of Probability & Its Applications, 14(1), 153-158.*

Finkel, S. E., Pérez-Liñán, A., Seligson, M. A. (2007). The Effects of US Foreign Assistance on Democracy Building, 1990–2003. *World Politics, 59(03), 404-439.*

Fisher, R. A. (1925). *Statistical Methods for Research Workers*. Edinburgh: Oliver& Boyd.

Gerring, J., Bond, P., Barndt, W., Moreno, C. (2005). Democracy and Growth: A Historical Perspective. *World Politics, 57(3), 323-64.*

Goldsmith, A. A. (2001). Foreign Aid and Statehood in Africa. *International organization, 55(01), 123-148.*

Guo, S., Fraser, M.W. (2015). *Propensity Score Analysis. Statistical Methods and Applications.* Thousand Oaks: SAGE Publications.

Hearn, J. (2000). Aiding democracy? Donors and Civil Society in South Africa. *Third World Quarterly, 21(5), 815-830.*

Heckelman, J. C. (2010). Aid and democratization in the transition economies. *Kyklos, 63(4), 558-579.*

Heckman, J. (1997). Instrumental Variables: A study of Implicit Behavioral Assumptions Used in Making Program Evaluations. *Journal of Human Resources, 32(3), 441-462.*

Hirschman, A. O. (1987). *Getting Ahead Collectively: Grassroots Experiences in Latin America*. New York: Pergamon Press.

Holland, P. W. (1986). Statistics and Causal Inference. *Journal of the American Statistical Association, 81(396), 945-960*.

Howell, J. (2000). Making Civil Society from the Outside - Challenges for Donors. *The European Journal of Development Research, 12(1), 3-22.*

Kempthorne, O. (1952). *The Design and Analysis of Experiments*. New York: John Wiley & Sons.

Knack, S. (2001). Aid Dependence and the Quality of Governance: Cross-country Empirical Tests. *Southern Economic Journal, 68(2), 310-329.*

Knack, S. (2004). Does Foreign Aid Promote Democracy? *International Studies Quarterly, 48(1), 251-266.*

Lewis, D., Kanji, N. (2009). *Non-governmental Organizations and Development*, New York: Routledge.

Linz, J, Stepan, A. (1997). Toward Consolidated Democracies. In Diamond, L. J. (Ed.). (1997). *Consolidating the Third Wave Democracies: Themes and Perspectives*, Baltimore: Johns Hopkins University Press.

Macy, R. M. (1972). Report on Trust Funds of the United Nations. Joint Inspection Unit, *JIU/REP/72/1*.

MacKellar, L., Ferreira, A., Tourres, E., Siri, J. (2014). UNDEF – Funded Projects on Rule of Law. *Cluster Evaluation Report. The United Nation Democracy Fund.*

Marshall, M.G., Cole, B.R. (2014). Global Report 2014. Conflict, Governance, and State Fragility. *Center For Systemic Peace, http://www.systemicpeace.org/vlibrary/GlobalReport2014.pdf.*

Marshall, M.G., Gurr, T.R, Jaggers, K. (2014). Polity IV Project. Political Regime Characteristics and Transitions, 1800-2013. *Center For Systemic Peace, http://www.systemicpeace.org/inscr/p4manualv2013.pdf* .

Mercer, C. (2002). NGOs, Civil Society and Democratization: a Critical Review of the Literature. *Progress in Development Studies, 2(1), 5-22.*

Neyman, J., (1935). Statistical Problems in Agricultural Experimentation. *Supplement to the Journal of the Royal Statistical Society, 2(2), 107-180*.

OECD. (2013). *Aid at a Glance. Flows of Official Development Assistance to and through Civil Society Organizations in 2011*. OECD Development Co-operation Directorate.

Persson, T., Tabellini, G. (2005). *The Economic Effects of Constitutions*. Cambridge: MIT Press.

Persson, T., Tabellini, G. (2009). Democratic Capital: The Nexus of Political and Economic Change. *American Economic Journal: Macroeconomics, 1(2), 88-126.*

Rahman, S. (2006). Development, Democracy and the NGO Sector: Theory and Evidence from Bangladesh. *Journal of Developing Societies, 22, 451.*

Rosenbaum, P. R. (2002). *Observational Studies*. New York: Springer.

Rosenbaum, P. R., Rubin, D. B. (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. *Biometrika, 70(1), 41-55*.

Rubin, D. B. (1974). Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies. *Journal of Educational Psychology, 66(5), 688.*

Sanyal, B. (1994). *Cooperative Autonomy: The Dialectic of State-NGOs Relationship in Developing Countries.* Geneva: International Institute for Labour Studies.

Savun, B., Tirone, D. C. (2011). Foreign Aid, Democratization, and Civil Conflict: How Does Democracy Aid Affect Civil Conflict? *American Journal of Political Science, 55(2), 233-246*.

Scott, J. M., Steele, C. A. (2011). Sponsoring Democracy: The United States and Democracy Aid to the Developing World, 1988–20011. *International Studies Quarterly, 55, 47-69.*

United Nations (2000). United Nations Millennium Declaration. Resolution Adopted by the General Assembly, *A/55/L.2.*

United Nations (2005). 2005 World Summit Outcome. Resolution Adopted by the General Assembly, *A/RES/60/1.*

Year Book of International Organizations (2014). *Guide to Global Civil Society Networks. 2014-2015*. The Union of International Associations.

Table 1.1 - Status of contributions by cumulative amount as at 8 May 2014

Country	*2005*	*2006*	*2007*	*2008*	*2009*	*2010*	*2011*	*2012*	*2013*	*2014*	*Cumulative Amount (US dollars)*
1 USA	10,000,000	7,920,000		7,920,000	3,000,000	4,500,000	5,000,000	4,755,000	4,581,000		*47,676,000*
2 India	5,000,000	5,000,000		5,000,000	5,000,000		5,000,000	4,710,000	1,852,543		*31,562,543*
3 Sweden		729,450	755,650		2,722,755	1,411,075	1,547,375	2,143,623	3,496,875	2,273,175	*15,079,998*
4 Germany	1,600,000	1,584,785	1,056,604	2,964,960	1,500,000	1,300,000	1,300,000	1,271,740	1,300,000		*13,878,088*
5 Japan			10,000,000							180,000	*10,180,000*
6 Qatar	2,000,000	4,000,000		4,000,000							*10,000,000*
7Australia	7,304,974				456,900	481,700	495,750		473,100		*9,212,424*
8 Spain		1,059,080	1,231,133	1,211,915	1,388,504	54,540	57,823		57,823	20,553	*5,023,547*
9 France	588,100	629,350	656,550	1,913,316	1,006,400						*4,793,716*
10 Italy			1,485,400	1,462,400							*2,947,800*
11 Republic of Korea		1,000,000									*1,000,000*
12 United Kingdom	609,350										*609,350*
13 Poland	50,000	50,000	100,000				30,000	158,510	129,504		*518,014*
14 Romania			294,260	128,600							*422,860*
15 Chile	20,000	80,000	30,000	30,000	30,000	30,000	30,000	30,000	30,000	30,000	*340,000*
16 Denmark		265,018									*265,018*
17 Turkey			50,000	25,000	25,000	25,000	25,000	25,000	30,000		*205,000*
18 Czech Republic	50,000		84,100	61,681							*195,780*
19 Portugal	50,000			100,000							*150,000*
20 Slovenia	30,000	30,000	20,000	20,000	27,924						*127,924*
21 Senegal	100,000										*100,000*
22 Hungary	25,000		25,000		25,000						*75,000*
23 Croatia	5,000		20,000	22,000	24,000						*71,000*
24 Israel		17,500	10,000	10,000	10,000	5,000	5,000				*57,500*
25 Peru			20,000	5,000				10,000	20,000		*55,000*
26 Lithuania			13,278	15,642			10,412	10,000			*49,332*
27 Georgia		24,943									*24,943*
28 Panama						7,000	5,000	5,000	5,000		*22,000*
29 Argentina							5,000	5,000		5,000	*15,000*
30 Estonia		10,395									*10,395*
31 Bulgaria			10,000								*10,000*
32 Libya										10,000	*10,000*
33 Mongolia		10,000									*10,000*
34 Cyprus			5,000								*5,000*
35 Ecuador				5,000							*5,000*
36 Iraq						5,000					*5,000*
37 Latvia				5,000							*5,000*
38 Madagascar						5,000					*5,000*
39 Morocco					5,000						*5,000*
40 Sri Lanka	5,000										*5,000*
Total	*27,437,424*	*22,410,520*	*15,866,974*	*24,900,514*	*15,221,503*	*7,824,315*	*13,453,537*	*13,181,696*	*11,938,575*	*2,498,175*	*154,733,232*

Source: UNDEF

Table 1.2 - UNDEF Projects implemented by civil society or CSOs at local level

		2006	*2007*	*2008*	*2009*	*2010*	*2011*	*2012*	*2006-2012*
Number of Countries		53	60	46	43	53	60	40	111
Number of civil society/NGOs Projects over the total number of projects		60/95	59/61	47/48	47/48	54/54	61/62	37/37	365/405
Amount approved (in dollars)	*Mean*	319,595	296,810	280,675	280,366	227,978	205,957	251,250	266,090
	Min	50,000	90,000	125,000	60,000	125,000	60,000	230,000	50,000
	Max	350,000	400,000	400,000	400,000	500,000	360,000	275,000	500,000

Source: own calculations from UNDEF projects database

Table 1.3 - Data description and sources

Variable	*Description*	*Source*
Democracy	Revised Combined Polity IV score ranging from -10 (hereditary monarchy) to +10 (consolidated democracy).	Centre for Systemic Peace
GDP per capita	Real GDP. US Dollars at constant prices (2005) and constant exchange rates (2005) per capita.	UNCTAD
ODA per capita	Total official development assistance net. US Dollars at current prices and current exchange rates in millions per capita.	UNCTAD
Urban Population	Percentage of urban population.	UNCTAD
Press Freedom	Freedom of the Press Index ranging from 0 (not free) to 100 (free).	Freedom House

Table 1.4 - Descriptive Statistics

Variable	*Obs.*	*Mean*	*Std. Dev*	*Min*	*Max*
Democracy	102	0.53	2.79	-10	12
GDP per capita (logged)	102	6.95	1.06	4.82	8.94
ODA per capita (logged)	102	3.06	1.17	0.14	5.51
Urban Population	102	44.59	19.99	8.30	93.61
Press Freedom	102	43.66	19.46	3.83	83.83

Table 1.5 - ATT matching estimate of CSOs projects on democracy

Variable	*Sample*	*Treated*	*Controls*	*Difference*	*S.E*	*T-stat*
Democracy	*Unmatched*	0.63	0.06	0.58	0.72	0.79
Countries benefiting for at least a round	*ATT*	0.72	-0.02	0.74	0.61	1.21
Democracy	*Unmatched*	0.74	0.14	0.60	0.58	1.04
Countries benefiting for at least two rounds	*ATT*	0.69	-0.59	1.29	0.86	1.50
Democracy	*Unmatched*	0.98	0.19	0.79	0.56	1.42
Countries benefiting for at least three rounds	*ATT*	0.98	-0.30	1.28	0.61	2.07

Table 1.6 - Covariate imbalance among treated and control countries before and after matching

	Unmatched	*Mean*			*%reduct*	*t-test*		*V(T)/*
Variable	*Matched*	*Treated*	*Controls*	*%bias*	|b*ias*|	*t*	*p>|t|*	*V(C)*
GDP	U	6.6984	7.1382	-42.3		-2.11	0.038	0.85
	M	6.6984	6.6759	2.2	94.9	0.10	0.918	0.91
ODA	U	2.944	3.1548	-18.1		-0.90	0.368	0.92
	M	2.944	3.0901	-12.5	30.7	-0.59	0.556	0.94
Urban Pop.	U	42.616	46.094	-17.3		-0.87	0.387	1.15
	M	42.616	43.59	-4.8	72.0	-0.23	0.821	1.15
Press Freedom	U	42.295	44.698	-12.6		-0.62	0.540	0.50*
	M	42.295	42.22	0.4	96.8	0.02	0.985	0.53*

** if variance ratio outside [0.55; 1.83] for U and [0.55; 1.83] for M*

Table 1.7 - ATT matching estimate using a probit model

Variable	*Sample*	*Treated*	*Controls*	*Difference*	*S.E*	*T-stat*
Democracy	*Unmatched*	0.63	0.06	0.58	0.73	0.79
Countries benefiting for at least a round	*ATT*	0.73	0.04	0.69	0.61	1.14
Democracy	*Unmatched*	0.74	0.14	0.60	0.58	1.04
Countries benefiting for at least two rounds	*ATT*	0.69	-0.65	1.34	0.86	1.56
Democracy	*Unmatched*	0.98	0.19	0.79	0.56	1.42
Countries benefiting for at least three rounds	*ATT*	0.97	-0.30	1.27	0.61	2.06

Table 1.8 - ATT estimates using nearest neighbour matching

Variable	*Sample*	*Treated*	*Controls*	*Difference*	*S.E*	*T-stat*
Countries benefiting for at least a round	*ATT n=5*	0.72	0.20	0.52	0.58	0.90
	ATT n=10	0.72	-0.02	0.73	0.55	1.35
	ATT n=20	0.72	0.06	0.66	0.50	1.33
Countries benefiting for at least two rounds	*ATT n=5*	0.69	-0.45	1.14	0.77	1.48
	ATT n=10	0.69	-0.23	0.93	0.70	1.32
	ATT n=20	0.69	0.04	0.65	0.67	0.98
Countries benefiting for at least three rounds	*ATT n=5*	0.98	-0.45	1.42	0.65	2.21
	ATT n=10	0.98	-0.39	1.37	0.63	2.18
	ATT n=20	0.98	-0.27	1.25	0.61	2.06

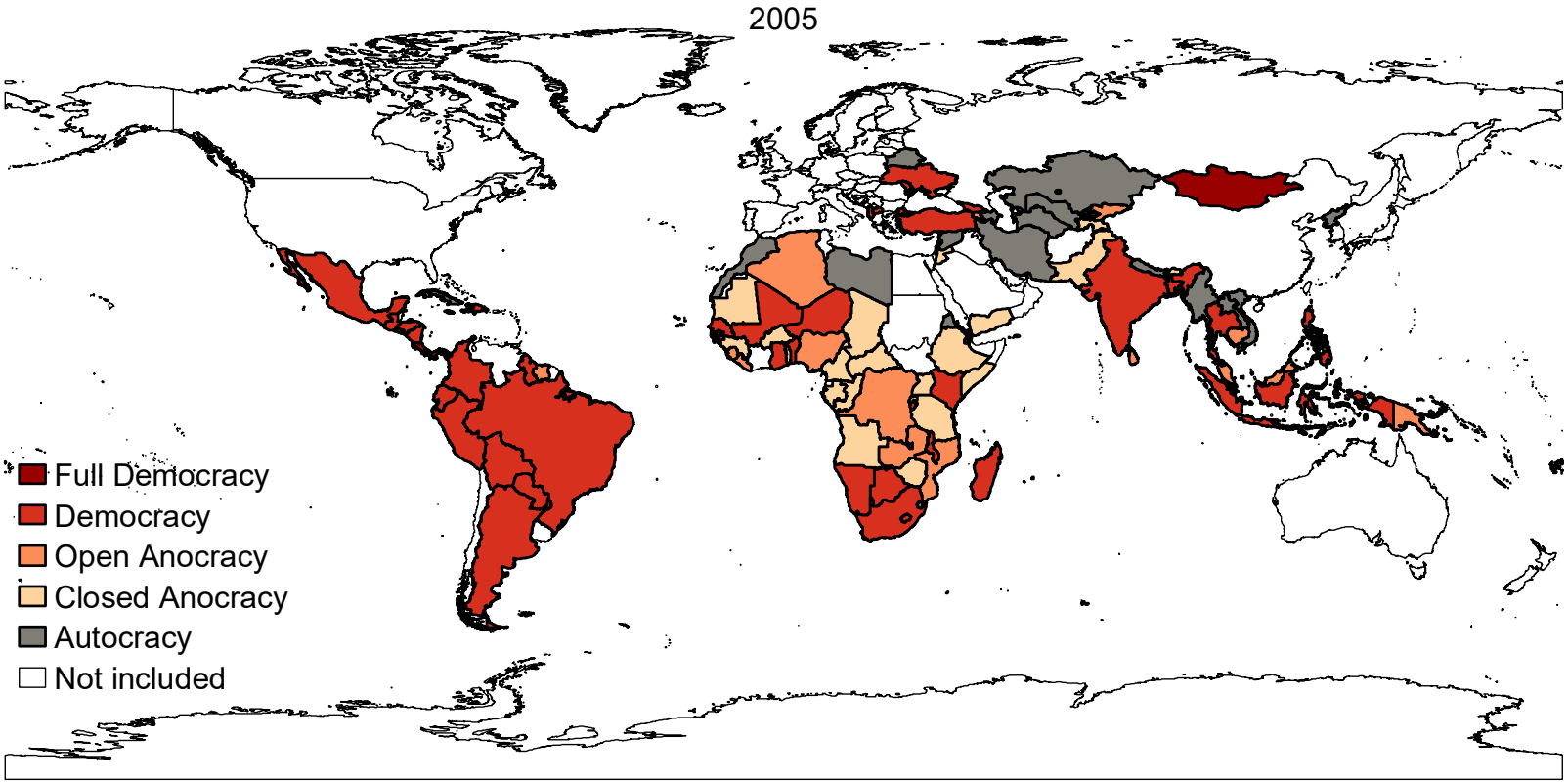
Table 1.9 - ATT estimates using the normal and the tricube kernel functions

Variable	*Sample*	*Treated*	*Controls*	*Difference*	*S.E*	*T-stat*
Countries benefiting for at least a round	*ATT normal kernel*	0.72	-0.04	0.75	0.54	1.41
	ATT tricube kernel	0.73	0.13	0.59	0.62	0.94
Countries benefiting for at least two rounds	*ATT normal kernel*	0.69	-0.45	1.14	0.73	1.55
	ATT tricube kernel	0.69	-0.63	1.32	0.88	1.51
Countries benefiting for at least three rounds	*ATT normal kernel*	0.98	-0.26	1.24	0.60	2.06
	ATT tricube kernel	0.98	-0.30	1.28	0.62	2.06

Table 1.10 - ATT estimates using different bandwidth parameters

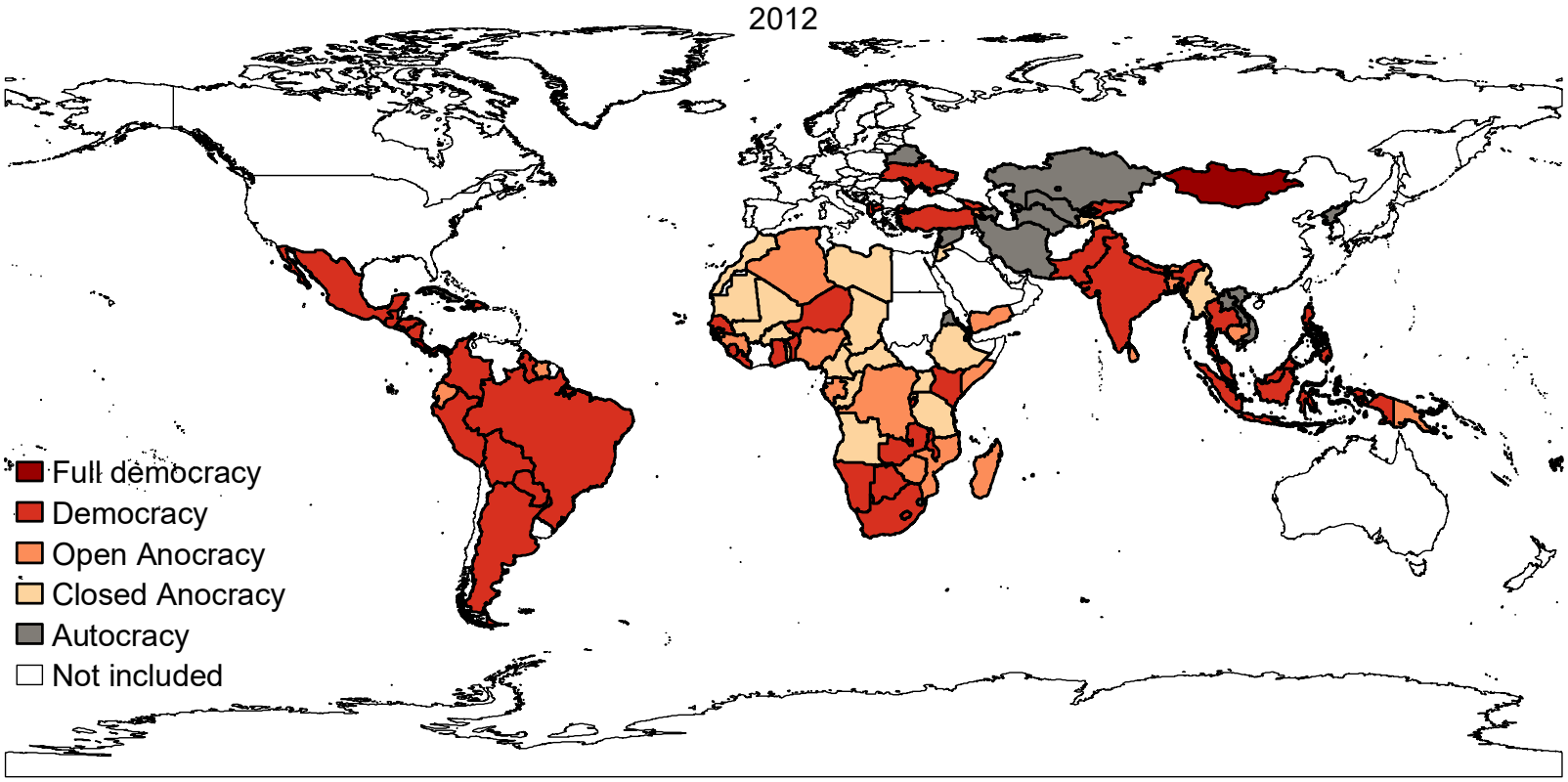
Variable	*Sample*	*Treated*	*Controls*	*Difference*	*S.E*	*T-stat*
Countries benefiting for at least a round	*ATT bw=0.04*	0.73	0.06	0.46	0.68	0.67
	ATT bw=0.08	0.72	-0.13	0.84	0.58	1.44
Countries benefiting for at least two rounds	*ATT bw=0.04*	0.72	-0.75	1.47	0.89	1.65
	ATT bw=0.08	0.69	-0.57	1.26	0.82	1.53
Countries benefiting for at least three rounds	*ATT bw=0.04*	0.98	-0.29	1.27	0.62	2.04
	ATT bw=0.08	0.98	-0.29	1.27	0.61	2.08

Figure 1.1 - Polity IV regimes in 2005



Source: own calculations from Polity IV Projects data

Figure 1.2 - Polity IV regimes in 2012



Source: own calculations from Polity IV Projects data

Figure 1.3 - Treated and control countries considering at least a round

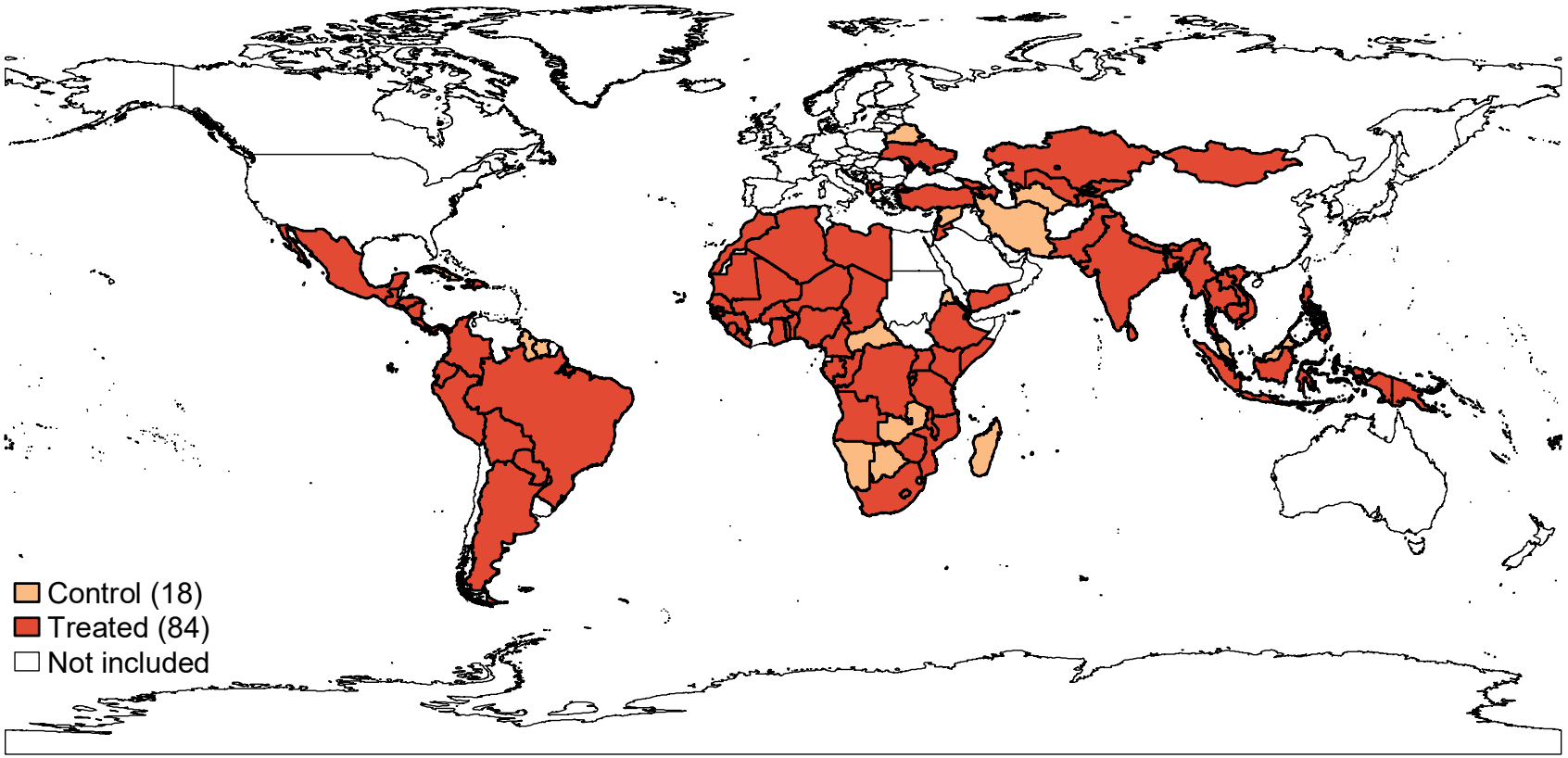


Figure 1.4 - Treated and control countries considering at least two rounds

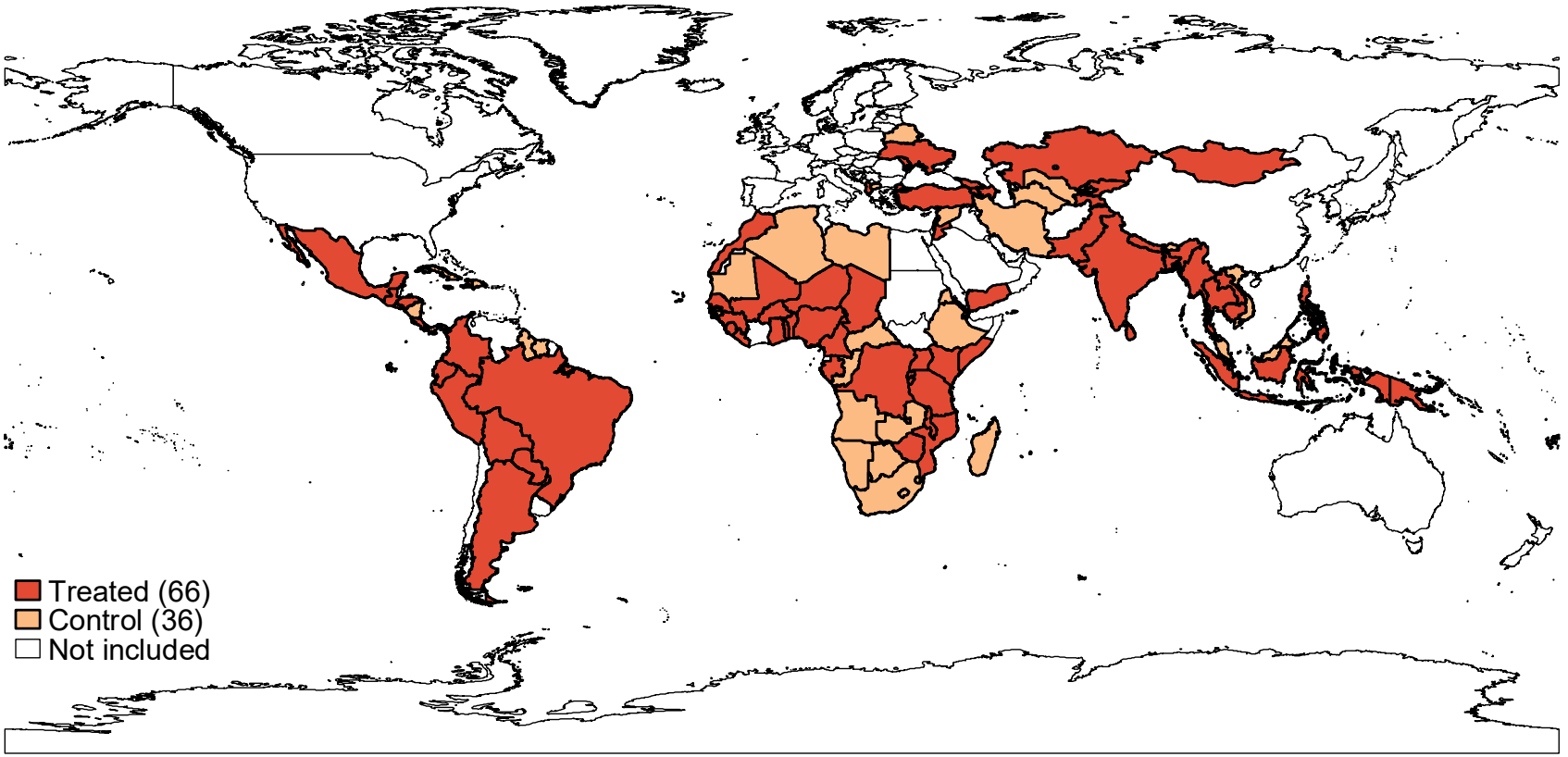
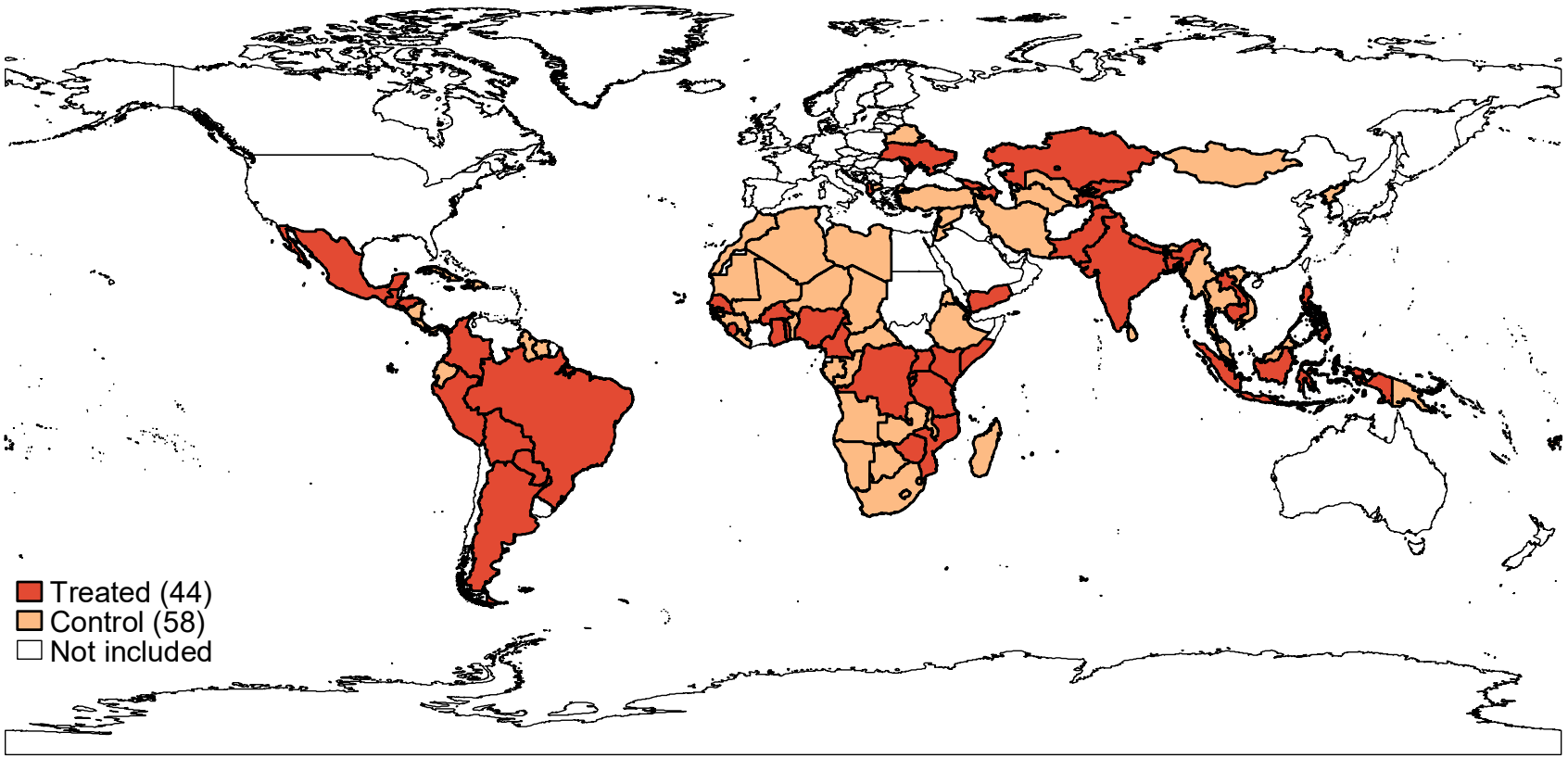


Figure 1.5 - Treated and control countries considering at least three rounds



Source: own calculations from UNDEF projects database

Figure 1.6 - Distribution of treated and control countries on the basis of the propensity score considering at least three rounds

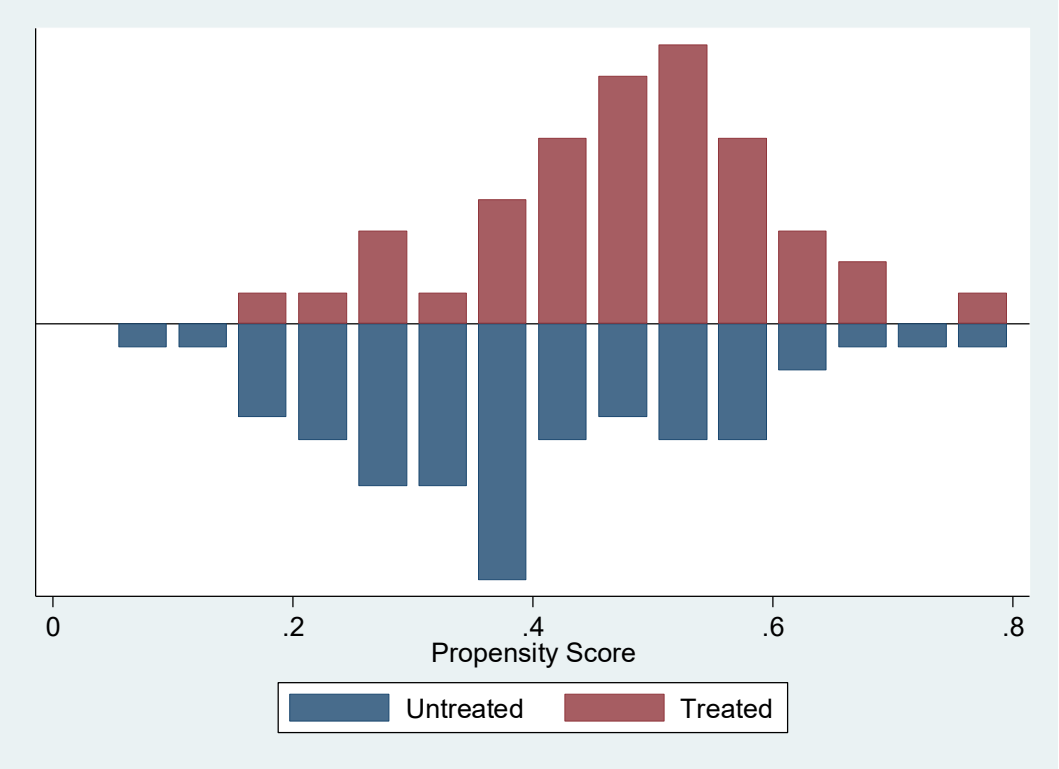


Figure 1.7 - Boxplots of pre-matching estimated propensity score for treated and control countries considering at least three rounds

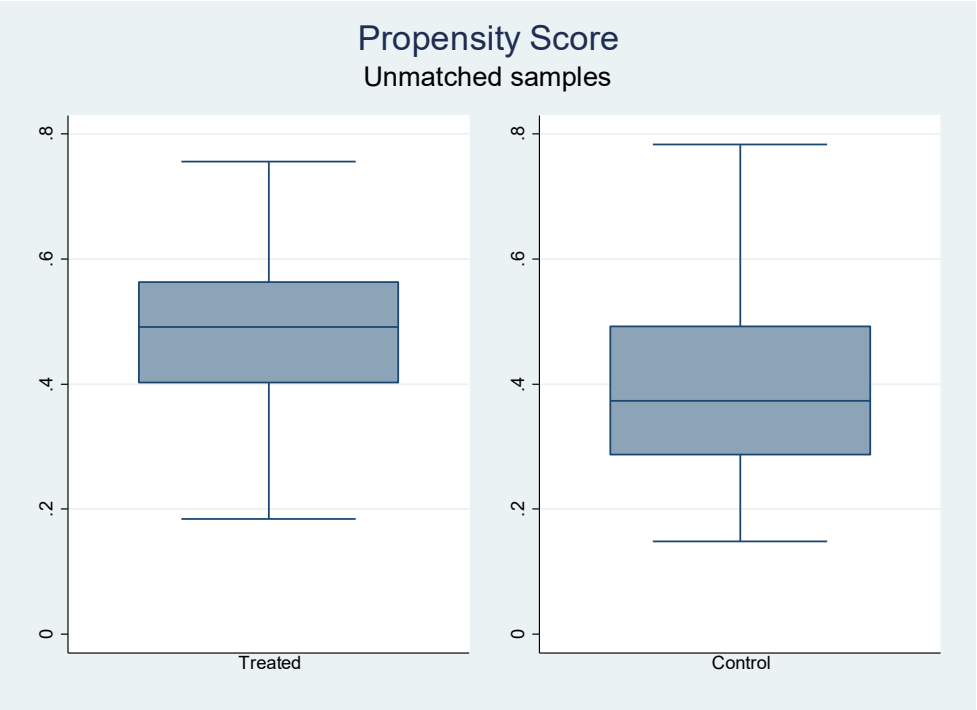
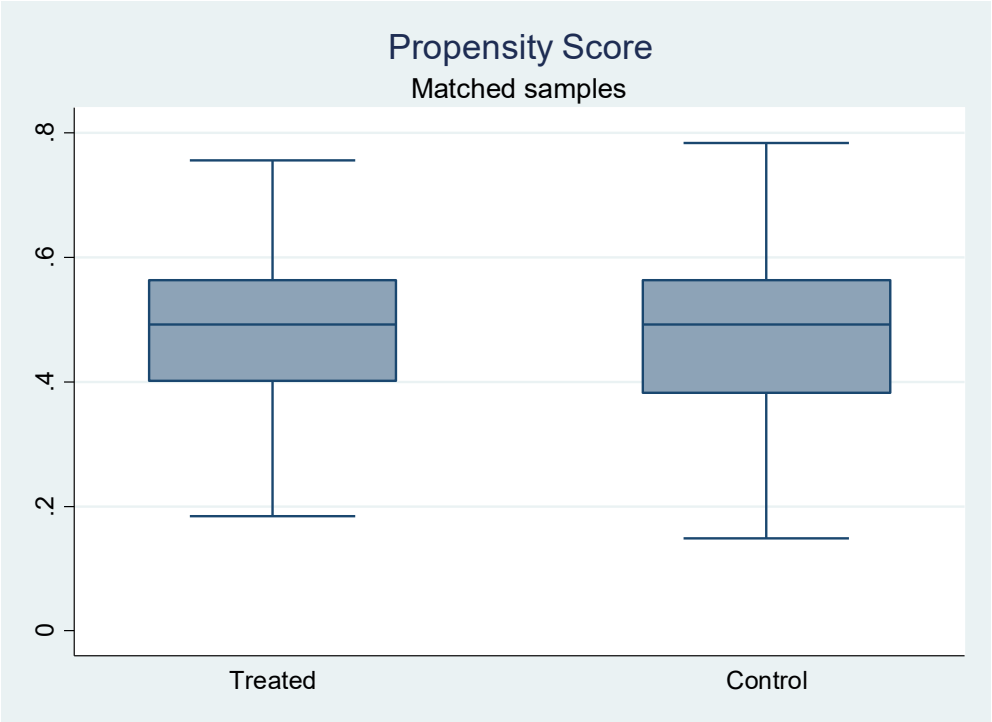


Figure 1.8 - Boxplots of post-matching estimated propensity score for treated and control countries considering at least three rounds



Appendix

Table 1.A.1 - UNDEF projects implemented by civil society or NGOs at local level

Country	2006 Total amount approved (dollars) and number of projects		2007 Total amount approved (dollars) and number of projects		2008 Total amount approved (dollars) and number of projects		2009 Total amount approved (dollars) and number of projects		2010 Total amount approved (dollars) and number of projects		2011 Total amount approved (dollars) and number of projects	
Afghanistan	350,000	1	400,000	1	350,000	1	135,000	1	300,000	1		
Albania			300,000	1	180,000	1			150,000	1		
Algeria							175,000	1				
Angola							325,000	1				
Argentina	300,000	1	100,000	1	325,000	1						
Armenia	280,997	1									200,000	1
Azerbaijan			200,000	1			200,000	1	220,000	1	225,000	1
Bangladesh			150,000	1	325,000	1	275,000	1	200,000	1	250,000	1
Belize									n/a	1		
Benin							300,000	1	250,000	1		
Bhutan					225,000	1						
Bolivia	350,000	1					350,000	1	n/a	1		
Bosnia and Herzegovina	279,759	1	150,000	1					125,000	1		
Brazil	350,000	1	300,000	1			250,000	1				
Bulgaria	113,085	1										
Burkina Faso					150,000	1			175,000	1	110,000	1
Burundi	302,450	1	225,000	1	500,000	2	500,000	2	250,000	1	200,000	1
Cabo Verde												
Cambodia	346,150	1	300,000	1			325,000	1	200,000	1	130,000	1
Cameroon			375,000	1			250,000	1	200,000	1	175,000	1
Chad			150,000	1							200,000	1
Chile					250,000	1			175,000	1		
China							505,000	2			225,000	1
Colombia			350,000	1	220,000	1	180,000	1				
Congo, Dem, Rep,	248,951	1	325,000	1	400,000	2					175,000	1
Congo, Rep, of			250,000	1								
Costa Rica	299,888	1									250,000	1
Cote d'Ivoire			125,000	1	250,000	1	225,000	1			200,000	1
Djibouti											200,000	1
Dominican Republic											n/a	1
Ecuador			350,000	1	400,000	1						
Egypt, Arab Rep,			350,000	1	300,000	1	250,000	1	n/a	1	n/a	1

Continued on next page

… table 1.A.1 continued

Country	2006 Total amount approved (dollars) and number of projects		2007 Total amount approved (dollars) and number of projects		2008 Total amount approved (dollars) and number of projects		2009 Total amount approved (dollars) and number of projects		2010 Total amount approved (dollars) and number of projects		2011 Total amount approved (dollars) and number of projects	
El Salvador	339,500	1	300,000	1							250,000	1
Ethiopia			400,000	1								
Fiji											n/a	1
Gabon					125,000	1					200,000	1
Gambia							175,000	1	225,000	1		
Georgia	333,550	1					165,000	1	n/a	1	115,000	1
Ghana	270,000	1	350,000	1	250,000	1	400,000	1	175,000	1		
Guatemala	591,675	2	150,000	1	300,000	1			175,000	1	n/a	1
Guinea			400,000	1					200,000	1		
Guinea-Bissau	350,000	1										
Haiti					350,000	1			225,000	1		
Honduras			400,000	1			300,000	1			225,000	1
India	589,218	2	350,000	1	350,000	1	320,000	1	725,000	2	250,000	1
Indonesia	208,301	2	300,000	1	350,000	1	225,000	1			200,000	1
Iraq	959,533	3	400,000	1	450,000	2	200,000	1	225,000	1		
Israel	349,540	1										
Jamaica			300,000	1	125,000	1						
Jordan			400,000	1					n/a	1		
Kazakhstan			300,000	1	175,000	1	200,000	1			250,000	1
Kenya			350,000	1	250,000	1	475,000	2	225,000	1	225,000	1
Kosovo	296,800	1	400,000	1	325,000	1	200,000	1			250,000	1
Kyrgyz Republic					300,000	1			225,000	1	120,000	1
Lao PDR							375,000	1	200,000	1	250,000	1
Lebanon					375,000	1			400,000	2	n/a	1
Liberia			300,000	1			180,000	1				
Libya											225,000	1
Macedonia, FYR			90,000	1								
Madagascar												
Malawi			300,000	1							175,000	1
Malaysia												
Maldives									264,000	1		
Mali	300,000	1	350,000	1								
Mauritania									225,000	1		
Mauritius							60,000	1				
Mexico					375,000	1	325,000	1	225,000	1	175,000	1

Continued on next page

… table 1.A.1 continued

Country	2006 Total amount approved (dollars) and number of projects		2007 Total amount approved (dollars) and number of projects		2008 Total amount approved (dollars) and number of projects		2009 Total amount approved (dollars) and number of projects		2010 Total amount approved (dollars) and number of projects		2011 Total amount approved (dollars) and number of projects	
Moldova	207,000	1			150,000	1			225,000	1	200,000	1
Mongolia					300,000	1					175,000	1
Morocco			250,000	1							275,000	1
Mozambique			300,000	1	350,000	1			250,000	1	200,000	1
Myanmar			300,000	1			250,000	1				
Nepal	350,000	1	350,000	1	300,000	1	275,000	1	275,000	1	n/a	1
Nicaragua											250,000	1
Niger			100,000	1							175,000	1
Nigeria	339,550	1	225,000	1	325,000	1	295,000	1	200,000	1		
Pakistan	232,300	1	325,000	1	225,000	1	250,000	1	475,000	2	225,000	1
Palestine	563,976	2	300,000	1	220,000	1	325,000	1	150,000	1	180,000	1
Panama	294,521	1										
Papua New Guinea	224,000	1	350,000	1								
Paraguay	273,000	1			250,000	1			200,000	1		
Peru	264,784	1					530,000	2			n/a	1
Philippines	349,125	1	300,000	1	250,000	1			225,000	1	200,000	1
Romania	267,375	1	250,000	1								
Russian Federation	224,000	1	350,000	1			405,000	2	175,000	1	200,000	2
Rwanda	256,375	1					385,000	2			100,000	1
Sao Tome and Principe	339,839	1										
Senegal	259,017	1	225,000	1	250,000	1			275,000	1		
Serbia	627,161	2	350,000	1					125,000	1		
Sierra Leone	698,959	2	350,000	1	250,000	1			225,000	1	360,000	2
Somalia			200,000	1			110,000	1	150,000	1		
South Africa	350,000	1										
South Sudan											n/a	1
Sri Lanka					225,000	1					n/a	1
Sudan	50,000	1							n/a	1		
Tajikistan	180,875	1							225,000	1	175,000	1
Tanzania			300,000	1			225,000	1			n/a	1
Thailand	230,000	1	400,000	1								
Timor-Leste	80,740	1	300,000	1							275,000	1
Togo	302,680	1	325,000	1	250,000	1					225,000	1
Tonga											60,000	1

Continued on next page

… table 1.A.1 continued

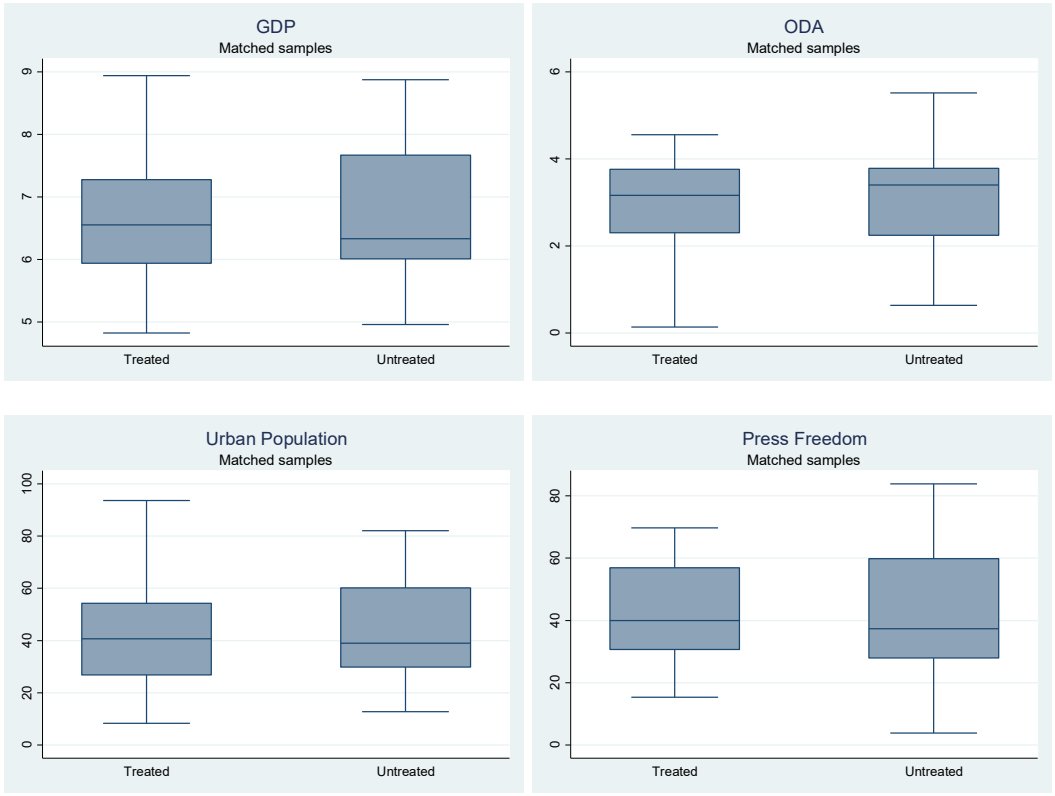
Country	*2006* *Total amount approved (dollars) and number of projects*		*2007* *Total amount approved (dollars) and number of projects*		*2008* *Total amount approved (dollars) and number of projects*		*2009* *Total amount approved (dollars) and number of projects*		*2010* *Total amount approved (dollars) and number of projects*		*2011* *Total amount approved (dollars) and number of projects*	
Trinidad and Tobago	262,500	1										
Tunisia									150,000	1	305,000	2
Turkey			350,000	1	230,000	1						
Turkmenistan												
Uganda	324,351	2	250,000	1	250,000	1			225,000	1	225,000	1
Ukraine	216,752	1			299,700	1	275,000	1	175,000	1	n/a	1
Uzbekistan											200,000	1
Vanuatu	107,776	1										
Vietnam									175,000	1		
Yemen	347,303	1	225,000	1			325,000	1	200,000	1		
Zimbabwe	266,000	1	300,000	1	250,000	1			200,000	1	225,000	1

Source: UNDEF projects database

Table 1.A.2 - List of treated and control countries considering at least three rounds

Country	*Status*	*Country*	*Status*	*Country*	*Status*
Albania	Treated	Gambia	Control	Myanmar	Control
Algeria	Control	Georgia	Treated	Namibia	Control
Angola	Control	Ghana	Treated	Nepal	Treated
Argentina	Treated	Guatemala	Treated	Nicaragua	Control
Armenia	Control	Guinea	Control	Niger	Control
Azerbaijan	Treated	Guinea Bissau	Control	Nigeria	Treated
Bangladesh	Treated	Guyana	Control	Pakistan	Treated
Belarus	Control	Haiti	Control	Panama	Control
Benin	Control	Honduras	Treated	Papua New Guinea	Control
Bhutan	Control	India	Treated	Paraguay	Treated
Bolivia	Treated	Indonesia	Treated	Peru	Treated
Botswana	Control	Iran	Control	Philippines	Treated
Brazil	Treated	Jamaica	Control	Rwanda	Treated
Burkina Faso	Treated	Jordan	Control	Senegal	Treated
Burundi	Treated	Kazakhstan	Treated	Sierra Leone	Treated
Cambodia	Treated	Kenya	Treated	Somalia	Treated
Cameroon	Treated	Kyrgyzstan	Treated	South Africa	Control
Cape Verde	Control	Lao PDR	Treated	Sri Lanka	Control
Central African Republic	Control	Lebanon	Treated	Suriname	Control
Chad	Control	Lesotho	Control	Swaziland	Control
Colombia	Treated	Liberia	Control	Syrian Arab Rep.	Control
Comoros	Control	Libya	Control	Tajikistan	Treated
Congo, Dem. Rep.	Treated	Macedonia, FYR	Control	Tanzania	Treated
Republic of the Congo	Control	Madagascar	Control	Thailand	Control
Costa Rica	Control	Malawi	Control	Togo	Treated
Cuba	Control	Malaysia	Control	Turkey	Control
Djibouti	Control	Mali	Control	Turkmenistan	Control
Dominican Republic	Control	Mauritania	Control	Uganda	Treated
Ecuador	Control	Mauritius	Control	Ukraine	Treated
El Salvador	Treated	Mexico	Treated	Uzbekistan	Control
Eritrea	Control	Moldova	Treated	Vietnam	Control
Ethiopia	Control	Mongolia	Control	Yemen	Treated
Fiji	Control	Morocco	Control	Zambia	Control
Gabon	Control	Mozambique	Treated	Zimbabwe	Treated

Figure 1.A.1 - Boxplot of the covariate imbalance among treated and control countries



2. Oil discoveries and democracy

Abstract

The effect of natural resources on political regimes is evaluated, using the synthetic control method to compare the evolution of the democracy level of countries that experienced giant oil discoveries with the weighted democracy level of countries that do not incur the same event and have similar pre-event characteristics. Focusing on 12 countries that have reached the peak of oil discoveries from the 1970s, the exogenous variation in oil endowment does not have the same effect on all countries. In most cases, the event has a negative effect in the long run, but countries with a high level of democracy in the pre-event period are not affected by the peak of oil discoveries. These results support heterogeneity and non-linearities claimed in recent theoretical literature.

Keywords: natural resources, oil discoveries, democracy, synthetic control method
JEL classification: P16, P48, C21, C23, O57

This essay is based on work with R. Ricciuti (University of Verona and CESifo). The work was carried out for the UNU-WIDER project on "Managing Natural Resource Wealth (M-NRW)" and an earlier version of this essay was published in the WIDER Working Paper 2016/57. It has benefited from the comments of participants at the following conferences and workshops: Institutional and Organizational Economics Academy, Institut d'Études Scientifiques de Cargèse; the "Conflicts & Institutions" Workshop, University of Genova; 16th Jan Tinbergen European Peace Science Conference, Catholic University of the Sacred Heart, Milan; XXVIII Annual Conference of the Italian Society of Public Economics (SIEP), University of Salento; 57th Annual Conference of the Italian Economic Association (SIE), University "L. Bocconi".

2.1 Introduction

Natural resources such as minerals, oil, and gas are a source of rent for a state. However, there is strong evidence that large endowments of natural resources may reduce economic growth and are associated with non-democratic regimes. This problem is often called a 'resources curse'.[1] The explanation for this perverse outcome is the public sector: politicians use rents from natural resources as a tool to remain in power. For example, a job in the public sector may be offered in exchange for a vote, or simply as acceptance of the status quo in a non-democratic regime. In addition, so-called *white elephant* projects (expensive, below capacity, and therefore unsustainable over time) are another example of waste of public monies accrued from extractive resources. Overall, the excessive expansion of the public sector leads to inefficiency. Moreover, a growing mining sector with high profits tends to attract capital, reducing its availability for investments in other industries. First, this lowers funds for other profitable but less politically linked companies; second, it reduces diversification and exposes the country to idiosyncratic shocks in the resource-abundant sector. Both can hamper economic growth in the medium term.

The phenomenon of a resource curse is not unanimously supported. For example, claims have been made that it can be escaped by countries with good institutions. This paper takes heterogeneity[2] across countries seriously: we perform a data-driven analysis that, instead of calculating the average effect of natural resources across countries, compares for each country the actual political regime with the counterfactual in the absence of a natural resource shock. In our analysis, this shock is when oil wealth reaches its peak and therefore the value of political incumbency is the highest, a central tenet in the 'rentier model' introduced in the next section. More precisely, we apply the synthetic control method (SCM) developed by Abadie and Gardeazabal (2003), and extended in Abadie et al. (2010), to deal with endogeneity from omitted variable bias by accounting for the presence

[1] For comprehensive reviews, see van der Ploeg (2011) and Ross (2015).

[2] Robinson et al. (2006: 451) claim: 'For every Venezuela and Nigeria, there is a Norway or a Botswana. A satisfactory model should explain why resources seem to induce prosperity in some countries but not others'.

of unobservable time-varying confounders. Moreover, it has the added advantage of transparency (as the weights identify the countries used to estimate the counterfactual outcome) as well as flexibility (as the set of potential controls can be appropriately restricted to make the underlying country comparisons more reasonable).

The choice of the natural resource measure is critical, as exogeneity is a pre-requisite for a meaningful claim of causality. Oil production, the typical measure of natural resource abundance, is imperfect because production is non-monotonic over the lifecycle of any oilfield. Therefore, this is a poor indicator of oil wealth. Following Tsui (2011), we use the exogenous variation in oil endowment to provide evidence that does not suffer from the endogeneity problem. In particular, we evaluate the effect of the peak of oil discoveries, defined as the point in time after which the rate of oilfield discoveries begins to decline. We argue that this event is more plausibly exogenous than the initial discovery of oil as it depends more on geological factors than on exploration. Our findings are in line with the literature suggesting that the effect of natural resources on democracy depends on the quality of institutions (Mehlum et al. 2006; Robinson et al. 2006). In countries with a high level of democracy (e.g. India and Colombia), it does not decline, as it does in countries with low levels of democracy.

The paper is organized as follows: Section 2 reviews the literature on natural resources and political regimes; Section 3 presents the methodology; in Section 4 data and some tests on exogeneity are introduced. Section 5 shows the results and robustness checks are presented in Section 6. Section 7 concludes.

2.2 Natural resources and political regimes

In contrast with the first wave of models (Krugman 1987; Sachs and Warner 1999), and those based on rent-seeking (Lane and Tornell 1996; Torvik 2002) involving an unconditional and negative relationship between resource abundance and growth, a fairly standard result is that countries with good institutions are able to use resource rents to improve their economic performance. This is because well-developed institutions have enough checks and balances to prevent the predatory behaviour of politicians and the unproductive use of government expenditure.

Democracy is the political system that seeks to maximise accountability and the peaceful change of government and we therefore take it to stand for 'good institutions'. The most common argument linking more oil to less democracy is the "rentier effect": an abundant flow of oil revenues enables incumbents to reduce taxes and/or increase patronage and public goods, making it possible for them to buy off a larger set of potential challengers and reduce dissent (Ross 2001). Moreover, higher non-tax revenues allow autocrats to reduce taxation and attenuate demands for greater accountability that would emerge from the imposition of heavier taxes or reduced subsidies (Brautigam et al. 2008).

In the rentier model resource wealth does not affect the preferences of rulers but it does influence their ability to act on these preferences. Robinson et al. (2006) and Caselli and Cunningham (2009) claim that higher resources increase the value leaders place on remaining in office because resource rents make incumbency more valuable, inducing a ruler to invest more in regime-preserving activities. There is substantial evidence that oil strengthens authoritarian governments and prevents a transition to democracy: Andersen and Aslaksen (2013) show that oil wealth prolongs the survival in office of authoritarian rulers, while Bueno de Mesquita and Smith (2010) find that natural resource rents help authoritarian leaders to avoid revolutionary threats and to survive them when they occur. Moreover, oil-rich autocrats may spend extra resource rents in repression (Cotet and Tsui 2013) and on the military to buy loyalty (Wright et al. 2015).

The impact of oil wealth on democracies is more ambiguous. A number of studies claim that oil has pro-democratic effects in democracies, because it makes governments more stable (reducing the likelihood of a transition to autocracy) or improves democracy scores (Smith 2004, Tsui 2011). A second group of studies finds no evidence that oil helps to stabilize democratic regimes (e.g., Andersen and Aslaksen 2013). Finally, other studies suggest that the effect of oil on democratic stability is conditional: it may stabilize democracies that are wealthy and have strong institutions but foster the breakdown of accountability in poorer democracies or ones with weaker institutions (e.g., Ross 2012).

The effect of 'good institutions' on the resource curse has been analysed in a number of papers that have emphasised accountability and rule of law as pillars for the effective use of natural resources.

According to Robinson et al. (2006), politicians tend to over-extract natural resources because they overly discount the future. This raises the value of incumbency and provides politicians with more resources to influence election results and increase resource misallocation in the economy. Countries with institutions that promote accountability and competence tend to benefit from resource booms, as these institutions reduce the perverse political incentives that such booms create. Similarly, as Mehlum et al. (2006) show, the quality of institutions determines whether countries avoid the resource curse. Taken together, these results conflict with Sachs and Warner's (1999) claim that institutions are irrelevant for the resource curse.

Natural resources make it more difficult for citizens to take collective action against a kleptocrat because they provide rulers with substantial resources to buy off opponents. Acemoglu et al. (2004) say that the success of a kleptocrat lies in his ability to use a divide-and-rule strategy. Members of society need to co-operate in order to depose the kleptocrat, but this co-operation may be neutralized by imposing punitive taxation on opponents and by redistributing the proceeds to allies. In equilibrium, all citizens are exploited and the kleptocrat remains unchallenged.

Empirical evidence for the political resource curse has focused mainly on the use of panel data, and the quality of institutions has often been proxied by corruption. The link goes from resource availability to corruption and rent-seeking via protection, exclusive licences to exploit and export resources given by the political elite to oligarchs in order to capture wealth and political power. Resource dependence is indeed strongly associated with a worse corruption perception index, which in turn is associated with lower growth (Mauro 1995), and natural resource wealth stimulates corruption among bureaucrats and politicians (Ades and Di Tella 1999). According to Bhattacharyya and Hodler (2010), natural resources induce corruption in countries that have been in a non-democratic regime for more than 60 per cent of the years since 1956. Along the same lines, Collier and Hoeffler (2009)

claim that high natural resource rents and open democratic systems slow growth unless there are sufficient checks and balances.

A new line of empirical research involves quasi-experimental studies. Vicente (2010) compares changes in perceived corruption in Sao Tome, which announced a significant oil discovery in 1997–99, with those in Cape Verde, with no oil. The two locations have a similar history, culture, and political institutions. He finds that corruption increased by almost 10 per cent after the announcement of the oil discovery and slightly decreased thereafter. In a regression-discontinuity study not explicitly related to natural resources, Brollo et al. (2013) find that windfall government revenues in Brazilian municipalities increase corruption and strengthen incumbency, but adversely affect the quality of politicians.

Our analysis follows the insights of Tsui (2011) and Lei and Michaels (2014) in choosing oil discoveries as the main variable related with the resource curse is concerned. Tsui (2011) argues that oil production—the typical measure of natural resource abundance—is noisy. Owing to geological constraints, the production rate is non-monotonic over the lifecycle of an oilfield; therefore, production is not a good indicator of the remaining reserves and oil wealth (the capital value of future oil rent, and hence a stock variable). Moreover, production understates the oil wealth of swing producers who produce below their capacity. Oil exploration involves high risks: with the current technology, the success rate of exploration drilling is still below 50 per cent, and historically has been much lower (Cotet and Tsui, 2013). It is therefore plausible to treat oil discoveries as positive oil shocks, whose timing and size are more exogenous than oil production. Moreover, the size of the deposit, quality, and other cost-determining characteristics are exogenous.[3] Using an empirical strategy based on an instrumental variables approach, Tsui (2011) finds that larger oil discoveries cause slower transitions to democracy; however, there is no such effect in democratic countries. This is positively correlated with oil quality and negatively correlated with exploration and extraction costs. Lei and Michaels (2014) focus on the discovery of giant oilfields,

[3] Moreover, there is empirical evidence that bad political conditions lead to less oil exploration and production (Bohn and Deacon, 2000; Ross 2012). Hence, oil levels tend to be biased downward in countries with less democracy and more conflict, reducing the risk of positive bias.

each of which contained ultimate recoverable reserves of 500 million barrels equivalent or more before extraction began. They show that in a panel of countries, controlling for country and time fixed effects, the timing of giant oilfield discoveries is plausibly exogenous, at least in the short-medium run. They do not analyse the effect of oil discoveries on democracy, but find that giant oilfield discoveries increase the incidence of internal armed conflicts by about 5-8 percent within 4-8 years of discovery.[4]

For comparability, we use the variable devised by Tsui (2011), but take a fundamentally different approach. His method produces average effects of oil discoveries on the level of democracy, whereas ours gives the effect in each treated country. Therefore, his approach is more general but conceals differences across countries. Our methodology returns the country-specific effect at the price of concentrating on a few cases. The approaches are complementary.

Our methodology was applied, in addition to panel difference-in-differences, in a study by Smith (2015) using resource discovery in countries that were not previously resource-rich as a plausible exogenous source of variation. He finds a positive effect on gross domestic product (GDP) per capita levels in non-OECD (Organization for Economic Co-operation and Development) countries, and conflicting evidence of the long-run positive effect of resources on productivity, capital formation, and education.

2.3 The synthetic control approach

The SCM provides quantitative inference in small-sample comparative studies by estimating the counterfactual situation of one or several aggregate entities in the absence of an event or intervention (Abadie and Gardeazabal 2003; Abadie et al. 2010). The missing counterfactual outcome is given by the weighted outcome of all

[4] There are a number of differences between these approaches. First, Tsui includes only oil discoveries, whilst Lei and Michaels (2014) also include natural gas and condensate. Second, Cotet and Tsui (2013) aggregate reserves from all discoveries within a country in a given year, while Lei and Michaels (2014) use only the largest single discovery. According to Lei and Michaels (2014), the correlation between their indicator for giant oilfield discoveries and the indicator used by Cotet and Tsui (2013) is around 0.55.

potential comparison units that best reproduces the characteristics of the case of interest (Abadie et al. 2015). In our case, we compare the democracy level of countries with a peak of oil discoveries with the weighted democracy level of countries where the peak of oil discoveries has not been reached but with similar pre-event characteristics.

To frame the SCM in the context of the present study, assume that there is a balanced panel of I+1 countries indexed by i and observed over T years. Among these, country i=1 (treated unit) reaches the peak of oil discoveries at time $T_0<T$; the remaining I countries are not affected by giant oil discoveries (donor pool). The effect of the event is given by:

$$\alpha_{1t} = Y_{1t} - Y_{1t}^{N} \qquad [2.1]$$

where $t>T_0$, Y_{1t} is the observed outcome of country i=1 for a post-event period t, and Y_{1t}^{N} is the unobservable potential outcome of country i=1, the democracy level that would have been observed in the absence of the event. SCM estimates Y_{1t}^{N} by defining a weighted average of the donor pool (synthetic control). The estimator of α_1 at time t is given by the difference between the outcome of the treated unit and the outcome of the synthetic control at that period:

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{i=2}^{I+1} w_i^{*} Y_{it} \qquad [2.2]$$

The weights w_i^{*} are chosen such that the characteristics (*predictors*) of the treated unit are best reproduced by the characteristics of the synthetic control. More formally, let X_{1k} be the pre-event value of the kth democracy predictor for the treated unit, and let X_{0k} be a (1×I) vector of the pre-event values of the same variable kth for the units in the donor pool. Then, the vector $\boldsymbol{W^{*}}$ containing the weights assigned to each control unit is chosen in order to minimise the following sum:

$$\sum_{k=1}^{k} v_k \left(X_{1k} - X_{0k} W\right)^2 \qquad [2.3]$$

subject to $w_i \geq 0$ and $\sum_{i=2}^{I} w_i = 1$.[5] Here, v_k is a weight that reflects the predictive power of variable k. In the following analysis, we choose the positive semi-definite and diagonal matrix $\boldsymbol{V}$ using the data-driven procedure implemented by Abadie and Gardeazabal (2003) and Abadie et al. (2010): $\boldsymbol{V}$ minimises the mean squared prediction error (MSPE) of the outcome variable in the pre-event period. MSPE measures the expected squared distance between the outcome of the treated unit and the outcome of the synthetic control in the pre-event period.[6] Thus, the lower the MSPE, the more the synthetic control resembles the characteristic of the treated unit. To achieve lower MSPE, we implement the nested optimisation procedure that searches among all $\boldsymbol{V}$ matrices and sets of W weights for the best fitting convex combination of the units in the donor pool. Moreover, to ensure that the global minimum in the parameter space has been found, we run the nested optimisation using three different starting points of $\boldsymbol{V}$.[7]

This data-driven procedure reduces discretion in the choice of the comparison units and has the advantage of transparency by making explicit the relative contribution of each unit in the donor pool to the counterfactual outcome. In addition, SCM allows the unobserved variables affecting the outcome to change over time. When there is a large number of pre-event periods, only those units that are similar in both observed and unobserved characteristics should produce similar paths for the outcome under scrutiny. Therefore, if the trajectories of the democracy level of the treated unit and the synthetic control are alike over numerous years prior to the peak in oil discoveries, a divergence in the outcome variable in the following years should be interpreted as produced by the peak itself.

These conclusions cannot be validated by the traditional modes of statistical inference because of the small-sample nature of the data. However, Abadie et al. (2010) provide an alternative model of inference defined as 'placebo studies' based

[5] This restriction prevents extrapolation outside the support of the data. See Abadie et al. (2015) for a discussion of its relevance.

[6] $MSPE = (1/T_0)\sum_{t<T_0}\left(Y_{1t} - \sum_{i=2}^{I+1} w_i^* Y_{it}\right)$.

[7] The three starting points are the regression-based V, the equal V weights, and a third procedure that uses the Stata maximum likelihood search. The nested optimization procedure is implemented by the Stata module synth (Abadie et al. 2011).

on the premise that the impact of the event under analysis would be undermined if an estimated effect of similar or greater magnitude were obtained in cases where the intervention did not take place. In particular, placebo studies apply SCM to every country in the pool of potential controls. This aims to assess whether the estimated effect for the treated country is large compared to the effect in a country chosen at random. In this study, 'in-space placebo tests' are used to compare the estimated treatment effect for each country that reaches the peak of oil discoveries with all the (fake) treatment effects of the control countries, obtained from experiments where each control country is assumed to be affected by the same event in the same year as the treated country. If the estimated effect in the treated country is larger than most of the effects obtained by the (fake) experiments, it can safely be concluded that the baseline results are not driven randomly by chance. This means that if the path of the post-event level of democracy in our case studies falls well outside the distribution of placebo effects, we attribute that effect to the peak of oil discoveries.

2.4 Democracy, predictors, and event periods

We measure the level of democracy using the *polity2* indicator from the Polity IV dataset (Marshall et al. 2014), which provides a 21-point scale ranging from −10 (hereditary monarchy) to +10 (consolidated democracy). To scale down the variance and reduce the effect of outliers, we transform the variable to lie between 0 and 1, with 1 corresponding to the highest level of democracy.

The set of predictors includes factors the literature identifies as determinants of democracy. We take into account the relationship between political regimes and economic factors including the log of GDP per capita (*Gdp*).[8] We also include a set of additional variables related to economic development that may predict a country's democratic level (see Acemoglu and Robinson 2006; Barro 1999; Lipset 1959): the index of human capital (*human capital*); the sum of imports and exports

[8] Several studies corroborate the results of the seminal work of Lipset (1959) according to which economic development consolidates democracy. Real gross domestic product on the expenditure side is used, since it enables a comparison of living standards across countries and over time (Feenstra et al. 2015).

over GDP (*openness*); and the value added by the mining,[9] manufacturing, and primary sectors as a percentage of GDP. In addition, we consider the hostility level of interstate disputes (*hostility*), and the total amount of natural resource rents as a percentage of GDP (*total rents*), to control for the possible effects of both conflicts and natural resource rents. Finally, we include the average level of democracy calculated in the 10 years preceding the event under scrutiny, to capture the quality of pre-existing institutions.

Following Tsui (2011), we identify the year of the event exploiting the oil production and depletion dataset collected by Campbell (2006). This dataset contains information on the peak year of oil discoveries for the top 65 oil countries. We consider that year as the period in which the event under scrutiny takes place.

The predictors are averaged over a 10-year pre-event period,[10] and the path of the outcome variable is analysed until 2014. Owing to data availability, we restrict our analysis to countries affected by the peak in the 1970s or later.[11] We also exclude the developed countries that do not show any variation in the Polity score in the time span considered.[12] Table 2.1 shows the countries analysed and the year in which they reached the peak of oil discoveries, and Appendix Table 2.A.1 lists the events excluded. For each treated unit, the donor pool encompasses all the countries not affected by the event for which data are available. Table 2.2 provides the definitions, sources, and descriptive statistics of variables.

[Table 2.1 about here]

[9] The value added by the mining sector is obtained subtracting manufacturing from the variable '*mining, manufacturing, utilities*' taken from the UNCTAD (United Nations Conference on Trade and Development) database. The noise of utilities in the measurement of the mining sector is small (Caruso et al. 2014).

[10] Data on *total rents*, *mining*, *manufacturing*, and *primary* are available from 1970. Hence, the time span over which they are averaged is different from the 10-year pre-event period for those countries that reached the peak in the 1970s: Brazil, Cameroon, Chad, India, Malaysia, Mexico, Tunisia, and Vietnam.

[11] Angola, United Arab Emirates, Uzbekistan, and Yemen are excluded because of the lack of pre-event data.

[12] These countries are Denmark, Italy, the Netherlands, Norway, and the United Kingdom.

[Table 2 about here]

To check whether the characteristics that predict the democracy level are also able to predict the peak of oil discoveries, we run cross-sectional linear regressions.[13] The dependent variable is equal to 1 if the country reached the peak of oil discoveries after 1970, and 0 otherwise. Predictors are measured at 1970. Table 2.3 shows the results. All the predictors are insignificant except for *human capital* and *openness*, whose coefficients are both negative and significant at 1 and 10 per cent, respectively. However, considering multivariate regression, only the initial level of human capital is a significant predictor of the peak of oil discoveries. The reason for this unusual result may be our sample which does not include developed countries (such as the UK and Norway) that are rich in oil. These countries have a high level of human capital. However, this result does not invalidate our analysis since SCM enables countries that have pre-event characteristics dissimilar to the treated unit to be discarded.[14]

[Table 2.3 about here]

2.5 Results

As highlighted in the previous sections, the credibility of SCM hinges on its ability to match the pre-event outcome of the treated country with that of the synthetic control. Table 2.4 reports the predictor balance and the root mean square prediction error (RMSPE) for each of our case studies. The low values of the RMSPE confirm the strengths of the synthetic control estimator. However, the RMSPE is higher than 0.10 for Malaysia, Pakistan, and Thailand. Because such a magnitude is deemed too high for a good fit between the path of the outcome variable of the treated unit and its synthetic control, these countries are discarded in the following discussion.

[13] Smith (2015) uses linear regressions to show that oil discoveries do not depend on the initial characteristics that may affect future growth.

[14] As can be seen in Table A2, developed countries are not usually included in the synthetic control, and when they are, a very small weight (with the exception of Japan/for India) is used.

[Table 2.4 about here]

Figures 2.1–2.12 provide a graphic illustration of the results: panels (a) display the trajectories of democracy level in each country and their synthetic counterparts, whereas the panels (b) show the gap between the two. Appendix Table 2.A.2 lists the potential controls and the weight assigned to each country in the synthetic control.

[Figures 2.1 – 2.12 about here]

The main finding is that oil discoveries do not affect all countries in the same way. Most of the case studies present a negative outcome gap in the long run. Figure 2.2 shows that the level of democracy of Cameroon is slightly lower than the synthetic control after the peak of oil discoveries. This negative outcome gap increases consistently five years after the peak of oil discoveries.

Democracy in Chad jumps after the peak (Figure 2.3). However, this is because the a civil war started in 1979, two years after the peak, ending in 1982 (Collins and Burns 2013). The democracy scores of the Republic of Congo (Figure 2.5) and Sudan (Figure 2.10) are slightly lower than those of their synthetic controls corresponding with the peak, but are higher for a short period (five and four years, respectively) after the event. Nevertheless, in the long run, the level of democracy in both is lower than would have been the case if no peak of oil discoveries had occurred. Viet Nam's democracy score is constant during the post-event period, where its counterfactual would have moved towards democracy (Figure 2.12). Kazakhstan (Figure 2.8) has a negative outcome gap in the pre-event period, increasing two years after the event, albeit with a low magnitude (–0.15 in our transformed index).

For all of those countries, the placebo tests presented in Figure 2.13 confirm a significant negative effect of oil discoveries on democracy in the decades following the event. In contrast, oil discoveries affect the level of democracy in Brazil only in the short run (Figure 2.1).

Mexico and Tunisia do not show clear paths. In Mexico, the peak of oil discoveries seems to halt the improvement in democracy under way on the eve of the event (Figure 2.9). However, as in Tunisia (Figure 2.11), the outcome gap of the country is significantly negative. The null hypothesis of no effect cannot be rejected in two cases: Colombia (Figure 2.13d) and India (Figure 2.13g). Indeed, although for both countries the average effect is negative, their post-event levels of democracy do not fall well outside the distribution of placebo effects. Therefore, oil discoveries have no significant effect on democracy in India and Colombia. Interestingly, these are the only two countries with a high level of democracy in the pre-event period (>0.9).

Finally, one striking case is Gabon (Figure 2.6), the only country in which the peak of oil discoveries seems to have a positive effect on democracy. However, this is misleading because, after the event under scrutiny, the political institutions of the country were impacted by another shock in the form of violent demonstrations and strikes in the 1990s, leading to political reforms including the transformation of the political system to a multiparty democracy (Collins and Burns 2013). These events are not captured by the synthetic control, which resembles the characteristics of the treated unit only in the absence of further permanent shocks in the outcome.

Tables 2.5 and 2.6 are complementary. For each case study, the former sets out the average effect of oil discoveries calculated by averaging the distance between our transformed indicator of the treated country and the synthetic control every 5 years after the peak of oil discoveries. It shows a widespread and often sizably lower level of democracy afterwards, in the event of the peak of oil discoveries. Gabon is an exception a positive difference for all but one period. Mexico has a negative effect in the middle of the period under analysis, which is otherwise positive. The negative effect for India is very small and in Brazil it declines over time, although it remains negative.

Table 2.6 reports the average *polity2* scores and the value of the counterfactual in the same standard 21-point scale. In the absence of the peak of oil discovery, Cameroon would have been categorised as a democracy with a polity

score of 6 rather than an anocracy (-4 in the Polity dataset).[15] The Republic of Congo and Sudan show a similar strong difference at the end of the period under analysis (7 and 8 points of the *polity2* scale, respectively), and would have obtained democratic characteristics achieving a score between 5 and 6 in the Polity scale. In a similar fashion Viet Nam, which no change in its Polity level, would have improved over time, ending up in the democratic group. The pattern of Tunisia would have been similar, starting from a lower level, and ending as an anocracy. In contrast, Kazakhstan would not have democratised even in the absence of the peak of oil discoveries reaching a *polity2* score of -3.

After a drop in democracy level compared to the synthetic control equal to 10 points in the *polity2* scale, Brazil caught up with its counterpart ten years after the peak of oil discoveries. This is a nearly unique pattern, similar only to Mexico, in which the negative effect is concentrated at the beginning of the period under analysis and vanishes over time: in most cases, the negative effect grows as time goes by. Colombia and India share another distinctive pattern since they consistently score as democracies, however the former deteriorates and the difference with the counterfactual grows over time, and the latter has a small U-shaped pattern in *polity2* with a counterfactual with higher value starting from t_5. Gabon, which stands in Table 2.5 for the positive effect of the oil peak, is still be below 0 for most of the post-peak period, but the value of the index would have been far more negative. For Chad the peak of oil discoveries would not make much difference, worsening the democracy score by a little, ending at most as an open anocracy at the end of the period.

[Table 2.5 about here]

[15] Marshall and Cole (2014: 21) define anocracies as those "countries whose governments are neither fully democratic nor fully autocratic but, rather, combine an often incoherent mix of democratic and autocratic traits and practices." The Polity IV Project classifies regimes as follows: autocracy from -10 to -6 of the polity score; closed anocracy from -5 to 0; open anocracy from 1 to +5; democracy from +6 to +9; full democracy +10.

[Table 2.6 about here]

Overall, countries that reached the peak of oil discoveries with a relatively high level of democracy tend to be less affected by the decline in democracy itself, whereas existing autocratic rule is strengthened. This is in line with some of the testable hypotheses derived by Caselli and Tesei (2016).

2.6 Robustness checks

This section includes a robustness check to test the sensitivity of our main results to changes in the measurement of the democracy level. We implement the SCM using the Polyarchy dataset compiled by Vanhanen (2014). This dataset provides an index of democracy given by the combination of its two most important dimensions: the degree of competition (*competition*) and the degree of participation (*participation*). The former is measured by the smaller parties' share of votes cast in parliamentary or presidential elections, and the latter is measured by the percentage of the population which actually voted in these elections. The combined index of democracy (*democracy*) is obtained by multiplying the two indicators and dividing the product by 100 (Vanhanen 2000). We estimate the synthetic control using these three variables as outcomes. For each indicator, Table 2.7 sets out the average effect of oil discoveries calculated every five years after the peak of oil discoveries.[16]

The trends in outcome gaps show that the results of the previous analysis are robust. In particular, the path of *democracy* gap almost perfectly replicates the path given by *polity2*.[17] This is not true for Kazakhstan, whose average effect is positive until ten years after the peak of oil discoveries. However, this discrepancy could be explained by the fact that the synthetic control does not replicate the country in the pre-event period. Indeed, the RMSPE is equal to 2.107 for

[16] Graphs and placebo tests are omitted to save space, but are available upon request from the authors.

[17] The same cannot be said of participation. However, a low value of the root mean square prediction error (RMSPE) was not obtained in most cases, as proven by the difference between the treated units and the synthetic controls at t_0.

democracy. Colombia seems to have a significant, negative outcome gap, but only 14 years after the event. In addition, in this case, the RMSPE is high. This difference vanishes considering *participation* for which the RMSPE is lower.[18] Another exception is Mexico for which the effect of oil discoveries is negative and significant for both *democracy* and *competition*. The negative gap starts to decrease five years after the peak. Overall, the peak of oil discoveries, at the very least, delayed democratisation in Mexico.

[Table 2.7 about here]

2.7 Conclusions

This paper undertakes a case-study analysis to evaluate the effect of a variation in oil endowment on political regimes. We used the SCM to estimate the democracy level that would have occurred in the absence of the event. This approach overcomes the weaknesses of previous analyses, solving the problems of endogeneity. In particular, the choice of concentrating on oil wealth and not oil production makes the shock reasonably exogenous.

Overall, this paper confirms the idea that natural resources may be a curse or a blessing for a country, depending on the quality of its institutions (Mehlum et al. 2006; Robinson et al. 2006). In particular, the relationship between natural resources and democracy shows some non-linearities depending on the initial level of democracy itself. Indeed, only the democracy levels of India and Colombia, with democracy scores above 0.9 (8 in the original *polity2*), do not change significantly after the peak of oil discoveries. All other countries, with the exception of Gabon where a period of political reforms took place after the peak of oil discoveries, were negatively affected by the variation in oil endowment. This effect is sizable, with, on average, the counterfactual level of democracy in those countries 3 points higher on the 21-item scale of *polity2* without the peak of oil discoveries.

[18] RMSPE is 1.001 for democracy, 5.287 for competition, and 0.395 for participation.

A plausible explanation of these results is that, as the rate of discoveries starts to decline, incumbents enforce higher entry barriers to retain the remaining resources. This is prevented in democracies with higher levels of executive constraints.

References

Abadie, A., A. Diamond, and J. Hainmueller (2010). Synthetic control methods for comparative case studies: estimating the effect of California's tobacco control program. *Journal of the American Statistical Association*, 105(490): 493–505.

Abadie, A., A. Diamond, and J. Hainmueller (2011). SYNTH: Stata module to implement Synthetic Control Methods for comparative case studies, Statistical Software Components S457334, Boston College Department of Economics, revised 28 Jan 2014.

Abadie, A., A. Diamond, and J. Hainmueller (2015). Comparative politics and the synthetic control method. *American Journal of Political Science*, 59(2): 495–510.

Abadie, A., and J. Gardeazabal (2003). The economic costs of conflict: A case study of the Basque Country. *American Economic Review*, 93(1): 113–32.

Acemoglu, D., J.A. Robinson, and T. Verdier (2004). Kleptocracy and divide-and-rule: A model of personal rule. *Journal of the European Economic Association*, 2(2–3): 162–92.

Acemoglu, D., and J.A. Robinson (2006). *Economic Origins of Dictatorship and Democracy*. Cambridge: Cambridge University Press.

Ades, A., and R. Di Tella (1999). Rents, competition, and corruption. *American Economic Review*, 89(4): 982–93.

Andersen, J.J., and S. Aslaksen (2008). Constitutions and the resource curse. *Journal of Development Economics*, 87(2):227–46

Andersen, J.J., and S. Aslaksen (2013). Oil and political survival. *Journal of Development Economics*, 100(1):89–106

Barro, R.J. (1999). Determinants of democracy. *Journal of Political Economy*, 107(S6): S158–83.

Barro, R.J., and J.W. Lee (2013). A new data set of educational attainment in the world, 1950–2010. *Journal of Economic Development*, 108(4): 184–98.

Bhattacharyya, S., and R. Hodler (2010). Natural resources, democracy and corruption. *European Economic Review*, 54(4): 608–21.

Bohn, H., and R.T. Deacon (2000). Ownership risk, investment, and the use of natural resources. *American Economic Review*, 90(3):526–49

Brautigam, D., O-H. Fjeldstad, and M. Moore (2008). *Taxation and State-Building in Developing Countries: Capacity and Consent.* New York: Cambridge Univ. Press

Brollo, F., T. Nannicini, R. Perotti, and G. Tabellini (2013). The political resource curse. *American Economic Review*, 103(5): 1759–96.

Bueno de Mesquita, B., and A. Smith (2010). Leader survival, revolutions, and the nature of government finance. *American Political Science Review*, 54(4):936–50

Campbell, C.J. (2006). Regular conventional oil production to 2100 and resource based production forecast [Excel spreadsheets through end 2005, revised 2006 August 15]. Available at: http://www.hubbertpeak.com/campbell/ (accessed July 2015).

Caruso, R., J. Costa, and R. Ricciuti (2014). The probability of military rule in Africa, 1970 to 2007. In K. Wärneryd (ed.), *The Economics of Conflict: Theory and Empirical Evidence*. Cambridge, MA: MIT Press, pp. 105–26.

Caselli, F., and T. Cunningham (2009). Leader behaviour and the natural resource curse. *Oxford Economic Papers*, 61:628–650.

Caselli, F., and A. Tesei (2016). Resource windfalls, political regimes, and political stability. *The Review of Economics and Statistics*, 98: 573-590.

Collier, P., and A. Hoeffler (2009). Testing the neocon agenda: Democracy in resource-rich societies. *European Economic Review*, 53(3): 293–308.

Collins, R.O., and J.M. Burns (2013). *A History of Sub-Saharan Africa*. Cambridge: Cambridge University Press.

Cotet, A., and K.K. Tsui (2013). Oil and conflict: What does the cross country evidence really show? *American Economic Journal: Macroeconomics*, 5(1): 49–80.

Feenstra, R.C., R. Inklaar, and M.P. Timmer (2015). The next generation of the Penn World Table. *American Economic Review*, 105(10): 3150–82.

Krugman, P. (1987). The narrow moving band, the Dutch disease, and the competitive consequences of Mrs. Thatcher: Notes on trade in the presence of dynamic scale economies. *Journal of Development Economics*, 27(1–2): 41–55.

Lane, P.R., and A. Tornell (1996). Power, growth and the voracity effect. *Journal of Economic Growth*, 1(2): 213–41.

Lei, Y-H., and G. Michaels (2014). Do giant oilfield discoveries fuel internal armed conflicts? *Journal of Development Economics*, 110: 139–157.

Lipset, S.M. (1959). Some social requisites of democracy: economic development and political legitimacy. *American Political Science Review*, 53(1): 69–105.

Marshall, M.G., and B.R. Cole (2014). Global report 2014. Conflict, governance, and state fragility. Available at: http://www.systemicpeace.org/vlibrary/GlobalReport2014.pdf (accessed July 2015).

Marshall, M.G., T.R. Gurr, and K. Jaggers (2014). Polity IV project: Political regime characteristics and transitions, 1800–2013. Available at: http://www.systemicpeace.org/polity/polity4.htm (accessed July 2015).

Mauro, P. (1995). Corruption and growth. *Quarterly Journal of Economics*, 110(3): 681–712.

Mehlum, H., K. Moene, and R. Torvik (2006). Institutions and the resource curse. *Economic Journal*, 116(508): 1–20.

Palmer, G., V. D'Orazio, M. Kenwick, and M. Lane (2015). The MID4 data set: Procedures, coding rules, and description. *Conflict Management and Peace Science*, 32(2): 222-24.

Psacharopoulos, G. (1994). Returns to investment educations: A global update. *World Development*, 22(9): 1325–43.

Robinson, J., R. Torvik, and T. Verdier (2006). Political foundations of the resource curse. *Journal of Development Economics*, 79(2): 447–68.

Ross M.L. (2015). What have we learned about the resource curse? *Annual Review of Political Science,* 18: 239–59

Ross M.L. (2012). *The Oil Curse: How Petroleum Wealth Shapes the Development of Nations*. Princeton, NJ: Princeton University Press

Ross, M.L. (2001). Does oil hinder democracy? *World Politics*, 53(3): 325–361.

Sachs, J.D., and A.M. Warner (1999). The big rush, natural resource booms and growth. *Journal of Development Economics*, 59(1): 43–76.

Smith, B. (2015). The resource curse exorcised: evidence from a panel of countries. *Journal of Development Economics*, 116: 57–73.

Smith, B. (2004). Oil wealth and regime survival in the developing world, 1960–99. *American Journal of Political Science*, 48(2):232–46

Torvik, R. (2002). Natural resources, rent seeking and welfare. *Journal of Development Economics*, 67(2): 455–70.

Tsui, K.K. (2011). More oil, less democracy: evidence from worldwide crude oil discoveries. *The Economic Journal*, 121(551): 89–115.

van der Ploeg, F. (2011). Natural resources: curse or blessing? *Journal of Economic Literature*, 49(2): 366–420.

Vanhanen, T. (2000). A new dataset for measuring democracy, 1810–1998. *Journal of Peace Research*, 37(2): 251–65.

Vanhanen, T. (2014). Measures of democracy, 1810–2012 [computer file]. FSD1289, version 6.0 (2014-01-31). T. Vanhanen and K. Lundell [data collection]. Tampere: Finnish Social Science Data Archive [distributor].

Vicente, P.C. (2010). Does oil corrupt? Evidence from a natural experiment in West Africa. *Journal of Development Economics*, 92(1): 28–38.

World Bank (2015). *World Development Indicators 2015*. Available at: http://data.worldbank.org/data-catalog/world-development-indicators/wdi-2015 (accessed July 2015).

Wright, J., E. Frantz, and B. Geddes (2015). Oil and autocratic regime survival. *British Journal of Political Science*, 45: 287–306.

Table 2.1 - Case studies

Country	Peak of oil discoveries	Start year synth
Brazil	1975	1965
Cameroon	1977	1967
Chad	1977	1967
Colombia	1992	1982
Republic of Congo	1984	1974
Gabon	1985	1975
India	1974	1964
Kazakhstan	2000	1991
Malaysia	1973	1963
Mexico	1977	1967
Pakistan	1983	1973
Sudan	1980	1970
Thailand	1981	1971
Tunisia	1971	1961
Viet Nam	1975	1965

Table 2.2 - Variable definitions, sources, and descriptive statistics

Variable	Description	Source	Mean	SD	Min.	Max.
Democracy	Transformed revised combined Polity IV score (*Polity2*) ranging from 0 (hereditary monarchy) to 1 (consolidated democracy)	Polity IV Project, Center for Systemic Peace (Marshall et al. 2014)	0. 560	0.359	0	1
Gdp	Log $RGDP^e$ per capita (at chained purchasing power parity in million, 2005 USD prices)	Penn World Table 8.1 (Feenstra et al. 2015)	8. 055	1.160	5.219	11.325
Human capital	Index of human capital per person, based on years of schooling (Barro and Lee 2013) and returns to education (Psacharopoulos 1994)	Penn World Table 8.1 (Feenstra et al. 2015)	2.007	0.627	1.018	3.535
Total rents	Total natural resources rents (percentage of GDP)	World Development Indicators (World Bank 2015)	6.637	10.277	0	83.432
Mining	Value added by sectors of economic activity, annual, 1970–2013: mining and utilities (percentage of GDP)	United Nations Conference on Trade and Development (UNCTAD)	6.607	8.547	0	72.123
Manufacturing	Value added by sectors of economic activity, annual, 1970–2013: manufacturing (percentage of GDP)	UNCTAD	15.666	7.534	0.032	50.180
Primary	Value added by kind of economic activity, annual, 1970–2013: agriculture, hunting, forestry, fishing (percentage of GDP)	UNCTAD	21.050	15.869	0.034	80.510
Openness	Sum of import and exports over GDP (at constant national 2005 prices)	Penn World Table 8.1 (Feenstra et al. 2015)	0.691	0.482	0.039	4.605
Hostility	Hostility level of interstate dispute ranging from 0 (no dispute) to 5 (war)	Palmer et al. (2015)	0.853	1.591	0	5

Note: SD, standard deviation; min., minimum; max., maximum; GDP, gross domestic product.

Table 2.3 - Peak of oil discovery and democracy predictors in 1970

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Gdp	–0.030								0.067
	(0.033)								(0.085)
Human capital		–0.150***							–0.344**
		(0.056)							(0.149)
Total rents			0.001						–0.004
			(0.006)						(0.006)
Mining				0.000					–0.000
				(0.006)					(0.008)
Manufacturing					–0.002				–0.000
					(0.003)				(0.006)
Primary						0.000			–0.003
						(0.002)			(0.004)
Openness							–0.153*		–0.134
							(0.078)		(0.101)
Hostility								0.030	0.032
								(0.027)	(0.032)

Note: The dependent variable is an indicator equal to 1 if the country has reached the peak of oil discoveries since 1970, and 0 otherwise. Covariates are measured in 1970. Robust standard errors are in parentheses. ***$P<0.01$, **$P<0.05$, *$P<0.10$.

Table 2.4 - Predictor balance and root mean square prediction error (RMSPE)

Predictor	Case study	
	Brazil	Synthetic Brazil
Gdp	8.049063	8.494432
Human capital	1.423229	1.592064
Total rents	3.032327	2.624917
Mining	2.817145	4.866887
Manufacturing	29.25174	19.27867
Primary	11.87682	25.98486
Openness	0.1084437	0.27975
Hostility	0.4	3.0722
Average pre-discovery democracy	0.075	0.07599
RMSPE		0.001012
	Cameroon	Synthetic Cameroon
Gdp	7.177065	7.141711
Human capital	1.311305	1.384907
Total rents	4.055883	6.277098
Mining	1.17727	1.180156
Manufacturing	14.70772	11.03428
Primary	28.28982	44.37049
Openness	0.2701598	0.2741007
Hostility	0.4	0.406
Average pre-discovery democracy	0.125	0.12755
RMSPE		0.0075677
	Chad	Synthetic Chad
Gdp	7.265192	7.25084
Total rents	4.75743	4.746761
Mining	0.9221356	0.921185
Manufacturing	12.31258	12.28897
Primary	41.9175	41.82736
Openness	0.498415	0.4972167
Hostility	0.2	0.1984
Average pre-discovery democracy	0.07	0.07283
RMSPE		0.0336496
	Colombia	Synthetic Colombia
Gdp	8.731305	9.189705
Human capital	2.016568	2.145918
Total rents	6.595242	1.345631
Mining	6.283752	4.958925
Manufacturing	18.12654	21.17535
Primary	12.55478	7.596763
Openness	0.2025784	0.2305646
Hostility	1.2	1.2468
Average pre-discovery democracy	0.905	0.904515
RMSPE		0.0011125
	Republic of Congo	Synthetic Republic of Congo
Gdp	7.536801	7.582842
Human capital	1.667621	1.458849
Total rents	39.64646	21.13613
Mining	28.3415	19.28067
Manufacturing	9.006013	9.808753
Primary	13.46419	16.33363
Openness	1.111467	1.124935
Hostility	0.5	0.4995
Average pre-discovery democracy	0.125	0.12557
RMSPE		0.0149957

Continued on next page

… table 2.4 continued

Predictor	Case study	
	Gabon	Synthetic Gabon
Gdp	9.252937	7.839756
Human capital	1.59167	1.627161
Total rents	47.64735	12.65417
Mining	41.641	6.981598
Manufacturing	6.07867	15.45709
Primary	5.053038	27.86907
Openness	1.016262	1.320546
Hostility	0.1	0.1008
Average pre-discovery democracy	0.05	0.0508
RMSPE		0.00063
	India	Synthetic India
Gdp	7.055507	8.774614
Human capital	1.223596	2.135507
Total rents	2.316797	2.268751
Mining	2.117753	2.168657
Manufacturing	14.2285	26.663
Primary	42.94529	13.29678
Openness	0.1135095	0.2515587
Hostility	4	0.3451
Average pre-discovery democracy	0.95	0.94994
RMSPE		0.0005722
	Kazakhstan	Synthetic Kazakhstan
Gdp	8.751681	8.707081
Human capital	2.708643	2.067864
Total rents	17.93863	9.386802
Mining	12.41103	2.115997
Manufacturing	11.92521	16.18919
Primary	13.41587	21.08155
Openness	1.21744	1.947997
Hostility	0.7777778	0.1145556
Average pre-discovery democracy	0.3222222	0.32225
RMSPE		0.0248456
Predictor	Malaysia	Synthetic Malaysia
Gdp	7.909242	7.99283
Human capital	1.655252	1.69775
Total rents	6.420214	6.41138
Mining	9.681566	9.961237
Manufacturing	14.61374	14.594
Primary	28.48076	15.85045
Openness	0.6859536	0.5896284
Hostility	1.5	0.8885
Average pre-discovery democracy	0.85	0.834385
RMSPE		0.1645738
	Mexico	Synthetic Mexico
Gdp	8.904852	8.059699
Human capital	1.65393	1.987598
Total rents	3.48081	3.472643
Mining	8.50995	8.469963
Manufacturing	19.22363	22.49874
Primary	10.6091	23.63679
Openness	0.1206917	0.2188393
Hostility	0	0.5021
Average pre-discovery democracy	0.2	0.1995
RMSPE		0.0001902

Continued on next page

… table 2.4 continued

Predictor	Case study	
	Pakistan	Synthetic Pakistan
Gdp	7.341958	7.060531
Human capital	1.3101	1.353643
Total rents	4.194234	1.987898
Mining	4.095575	4.535334
Manufacturing	10.42744	9.951427
Primary	34.40184	33.75569
Openness	0.3474532	0.3627543
Hostility	2.1	2.0756
Average pre-discovery democracy	0.45	0.442475
RMSPE		0.3453261
	Sudan	Synthetic Sudan
Gdp	7.152358	7.239045
Human capital	1.137935	1.476076
Total rents	0.0002809	2.006756
Mining	1.892794	2.165256
Manufacturing	8.861436	10.98123
Primary	38.16304	38.15145
Openness	0.1021655	0.4820521
Hostility	1.5	1.5302
Average pre-discovery democracy	0.175	0.17774
RMSPE		0.046698
	Thailand	Synthetic Thailand
Gdp	7.784089	8.283527
Human capital	1.70597	1.761231
Total rents	2.306225	2.597524
Mining	2.669549	6.407429
Manufacturing	19.71048	18.29137
Primary	25.37276	15.79119
Openness	0.4776597	0.4599142
Hostility	3.9	3.4765
Average pre-discovery democracy	0.435	0.43146
RMSPE		0.1992042
	Tunisia	Synthetic Tunisia
Gdp	7.418731	7.490103
Human capital	1.199441	1.417062
Total rents	3.160351	3.295567
Mining	5.51892	1.988342
Manufacturing	8.745414	10.55169
Primary	15.21589	28.22424
Openness	0.6536856	0.4287062
Hostility	0.7	2.2609
Average pre-discovery democracy	0.055	0.056595
RMSPE		0.0142813
	Viet Nam	Synthetic Viet Nam
Gdp	6.545513	7.523913
Human capital	1.739625	1.747607
Total rents	0	1.698035
Mining	3.951625	5.941236
Manufacturing	16.07247	15.72458
Primary	42.62852	34.60567
Openness	0.6227122	0.5732201
Hostility	5	1.3198
Average pre-discovery democracy	0.13	0.135505
RMSPE		0.024323

Table 2.5 - Average effect of the peak of oil discoveries on the transformed *polity2* indicator

Country	t_0	t_5	t_{10}	t_{15}	t_{20}	t_{25}	t_{30}
Brazil	–0.251	–0.501	–0.394	0.036	–0.003	–0.018	–0.028
Cameroon	–0.003	–0.047	–0.159	–0.388	–0.431	–0.412	–0.354
Chad	0.061	0.369	0.133	–0.125	–0.228	–0.177	–0.239
Colombia	0.000	–0.060	–0.099	–0.104	–0.113	–0.113	—
Republic of Congo	–0.050	–0.062	0.076	–0.068	–0.256	–0.394	–0.403
Gabon	–0.001	0.029	0.212	0.199	0.196	0.265	0.482
India	0.001	–0.069	–0.063	–0.067	–0.086	–0.036	–0.020
Kazakhstan	–0.022	–0.111	–0.151	–0.151	—	—	—
Mexico	0.151	0.136	0.095	–0.108	–0.128	0.046	0.092
Sudan	–0.047	–0.083	–0.090	–0.163	–0.607	–0.634	–0.659
Tunisia	–0.005	0.029	0.224	–0.495	–0.554	–0.506	–0.395
Viet Nam	–0.015	0.000	0.002	–0.045	–0.087	–0.280	–0.267

Note: Dashes (—) indicate no estimation is available.

Table 2.6 - Average level of *polity2* and counterfactual scores after the peak of oil discoveries

Country	t_0	t_5	t_{10}	t_{15}	t_{20}	t_{25}	t_{30}
Brazil	-4	-4	-1.2	7.6	8	8	8
Syntethic Brazil	1.016	6.0172	6.6844	6.8816	8.0624	8.354	8.5648
Cameroon	-8	-7.8	-8	-7.2	-4	-4	-4
Syntethic Cameroon	-7.932	-6.8506	-4.8154	0.558	4.619	5.062	3.0846
Chad	-7	-0.6	-5	-6	-3.2	-2	-2
Syntethic Chad	-8.21	-7.9842	-7.6558	-3.4996	1.3578	1.69725	2.777
Colombia	9	7.8	7	7	7	7	—
Syntethic Colombia	9.001	8.9942	8.9744	9.0886	9.261	9.261	—
Republic of Congo	-8	-8	1.2	-1.6	-4.6	-4	-4
Syntethic Congo	-6.993	-6.765	-0.3294	-0.25	0.5118	3.84325	4.051
Gabon	-9	-8.4	-4	-4	-4	-2.25	3
Syntethic Gabon	-8.984	-8.984	-8.2496	-7.984	-7.9168	-6.472	-6.64
India	9	7.6	8	8	8	9	9
Syntethic India	8.978	8.976	9.2616	9.333	9.714	9.7125	9.4002
Kazakhstan	-4	-5.6	-6	-6	—	—	—
Syntethic Kazakhstan	-3.555	-3.384	-2.985	-2.985	—	—	—
Mexico	-3	-3	-3	0	3.6	7	8
Syntethic Mexico	-6.01	-5.7264	-4.902	2.1622	6.158	6.4925	6.1524
Sudan	-7	-5.6	1.4	-7	-7	-6.25	-3.6
Syntethic Sudan	-6.901	-6.185	-3.0838	2.8988	4.088	4.29975	4.2952
Tunisia	-9	-9	-8.8	-8	-5	-3.5	-3
Syntethic Tunisia	-8.708	-8.999	-8.8332	-7.103	-3.268	2.189	2.3324
Viet Nam	-7	-7	-7	-7	-7	-7	-7
Syntethic Viet Nam	-6.062	-5.348	-5.1956	-3.735	5.142	5.70025	6.1836

Note: Dashes (—) indicate no estimation is available.

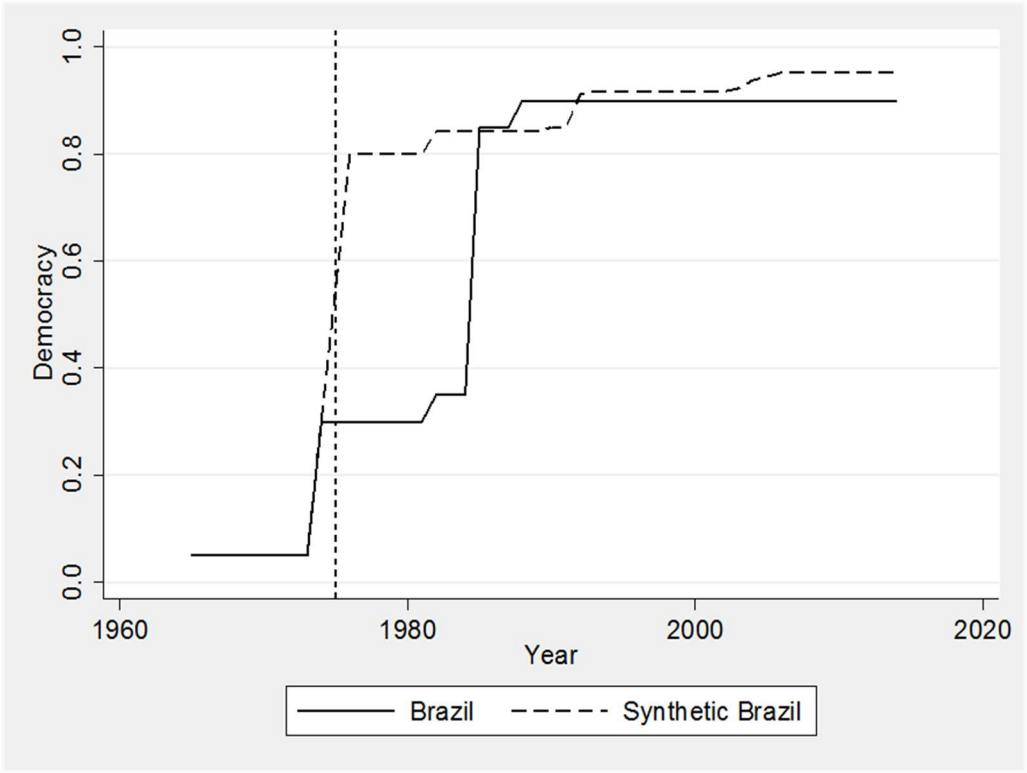
Table 2.7 - Average effect of the peak of oil discoveries on Vanhanen's democracy indicators

Country indicator	t_0	t_5	t_{10}	t_{15}	t_{20}	t_{25}	t_{30}
Brazil							
Democracy	0.000	–13.411	–14.853	–5.648	0.372	0.733	5.229
Competition	0.000	–3.078	9.581	10.624	9.703	12.685	14.509
Participation	–0.981	–1.844	–26.276	–12.368	12.092	14.725	22.829
Cameroon							
Democracy	–3.845	–6.663	–6.940	–6.650	–2.884	–12.007	–8.878
Competition	–1.347	–1.085	–8.257	–3.058	1.598	–34.089	–4.605
Participation	0.208	7.971	7.513	2.398	2.122	–3.600	–3.104
Chad							
Democracy	–5.461	–5.143	–4.765	–6.730	–10.362	–3.604	–2.873
Competition	–6.767	–6.489	–5.470	–21.258	–33.352	–3.709	10.240
Participation	–26.900	–25.384	–28.366	–27.397	–5.514	10.169	3.514
Colombia							
Democracy	–1.562	–4.373	0.414	–4.502	–7.993	—	—
Competition	1.693	1.306	0.260	–7.927	–19.607	—	—
Participation	–2.266	–11.189	–2.618	–8.047	–8.668	—	—
Republic of Congo							
Democracy	0.000	–2.927	10.520	3.802	0.242	–6.790	–9.382
Competition	0.000	–14.193	28.280	9.802	–1.358	–26.439	–37.315
Participation	–0.421	–0.496	17.558	–21.666	–1.117	13.766	–0.909
Gabon							
Democracy	–3.091	0.206	9.740	4.961	3.946	–4.843	–17.275
Competition	–3.280	2.446	37.020	26.051	23.792	0.731	–41.767
Participation	0.010	0.146	–1.362	–22.330	–11.963	–16.832	–25.646
India							
Democracy	–0.372	1.414	–0.171	–2.194	–4.754	–1.683	–4.023
Competition	3.114	4.609	1.768	–5.548	–4.287	1.367	–9.207
Participation	13.752	21.004	1.641	–4.640	6.472	11.376	12.496
Kazakhstan							
Democracy	4.658	3.418	–3.171	—	—	—	—
Competition	6.939	5.990	–10.113	—	—	—	—
Participation	1.511	–0.753	–3.049	—	—	—	—
Mexico							
Democracy	–19.508	–17.357	–11.245	–9.097	–6.149	–2.477	–2.231
Competition	–42.796	–38.662	–16.475	0.906	–0.624	5.809	10.322
Participation	–9.974	–5.826	–3.693	–12.637	3.498	1.506	–0.699
Sudan							
Democracy	–0.001	–12.316	–14.038	–13.647	–10.430	–14.149	–6.990
Competition	0.000	0.000	0.000	0.000	0.000	–0.800	7.240
Participation	1.144	2.190	0.509	–0.236	2.552	3.341	10.366
Tunisia							
Democracy	0.012	–0.190	1.121	–4.676	–0.679	–2.124	–13.120
Competition	–0.189	–0.593	13.474	–29.157	–9.099	–15.596	–25.757
Participation	0.110	–12.028	–31.423	–42.859	–20.544	–23.541	–44.198
Viet Nam							
Democracy	0.012	–0.190	1.121	–4.676	–0.679	–2.124	–13.120
Competition	–0.189	–0.593	13.474	–29.157	–9.099	–15.596	–25.757
Participation	0.110	–12.028	–31.423	–42.859	–20.544	–23.541	–44.198

Note: Dashes (—) indicate no estimation is available.

Figure 2.1 - Path of democracy: (a) Brazil versus synthetic control (b) outcome gap

(a)



(b)

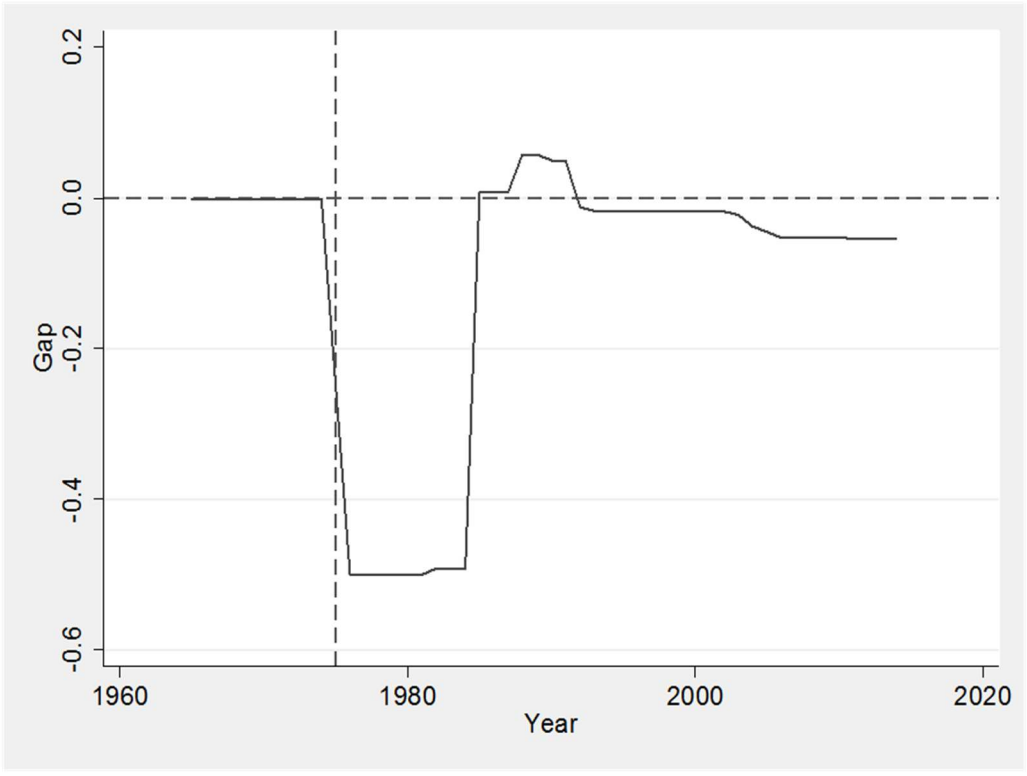
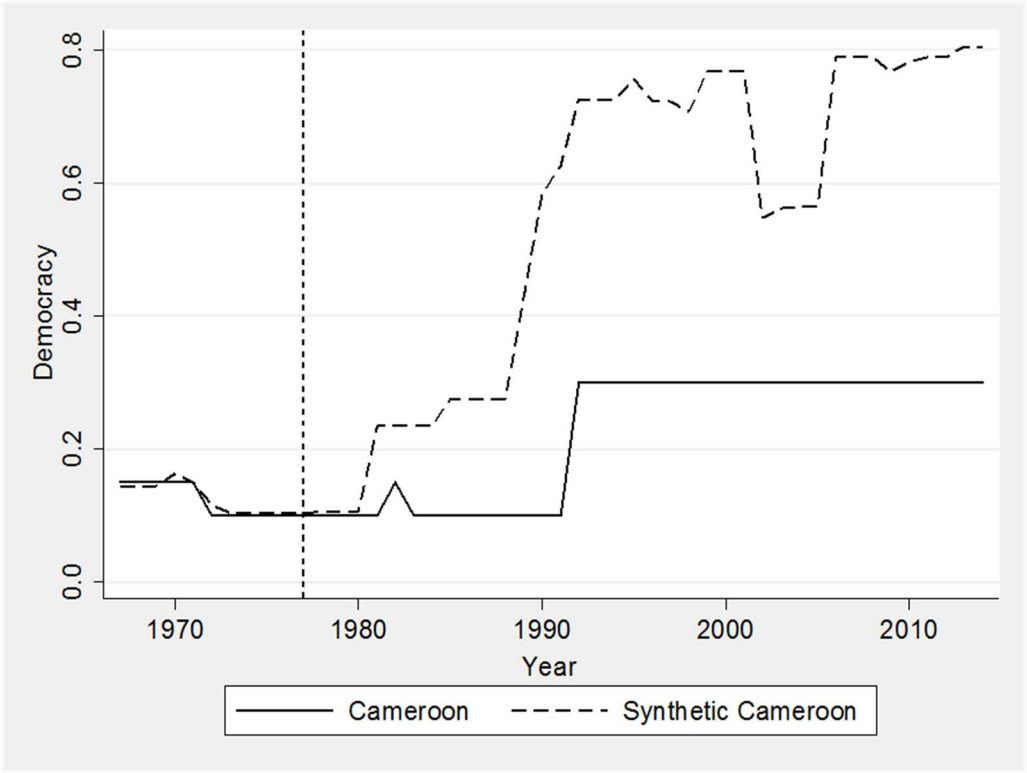


Figure 2.2 - Path of democracy: (a) Cameroon versus synthetic control; (b) outcome gap

(a)



(b)

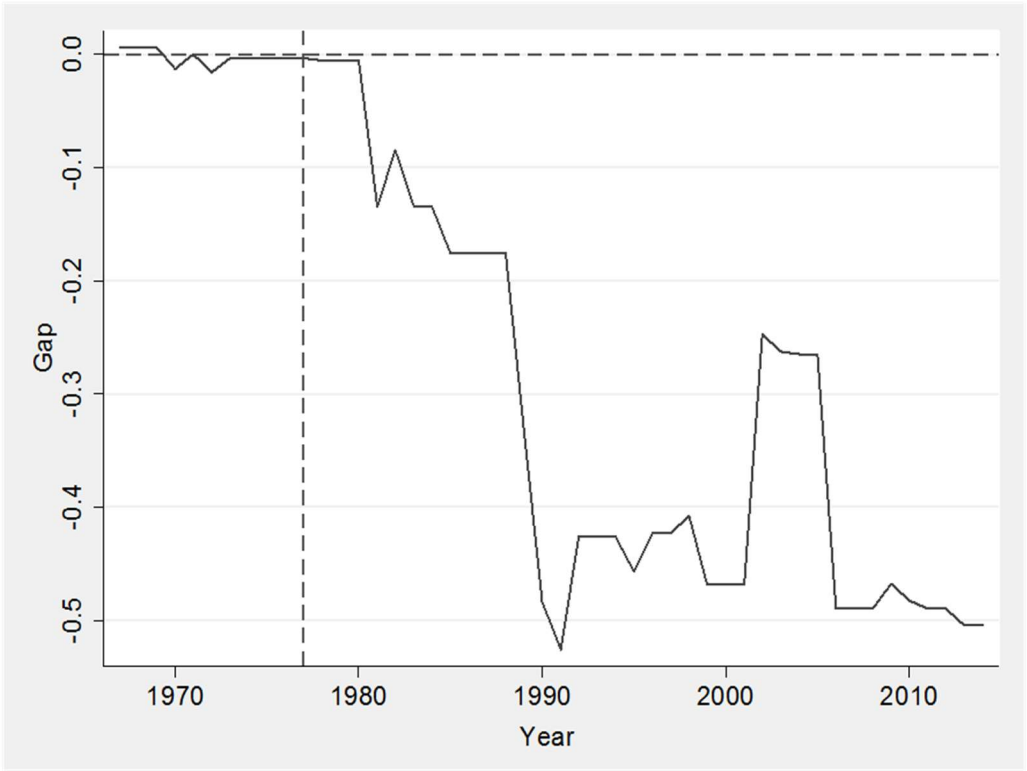
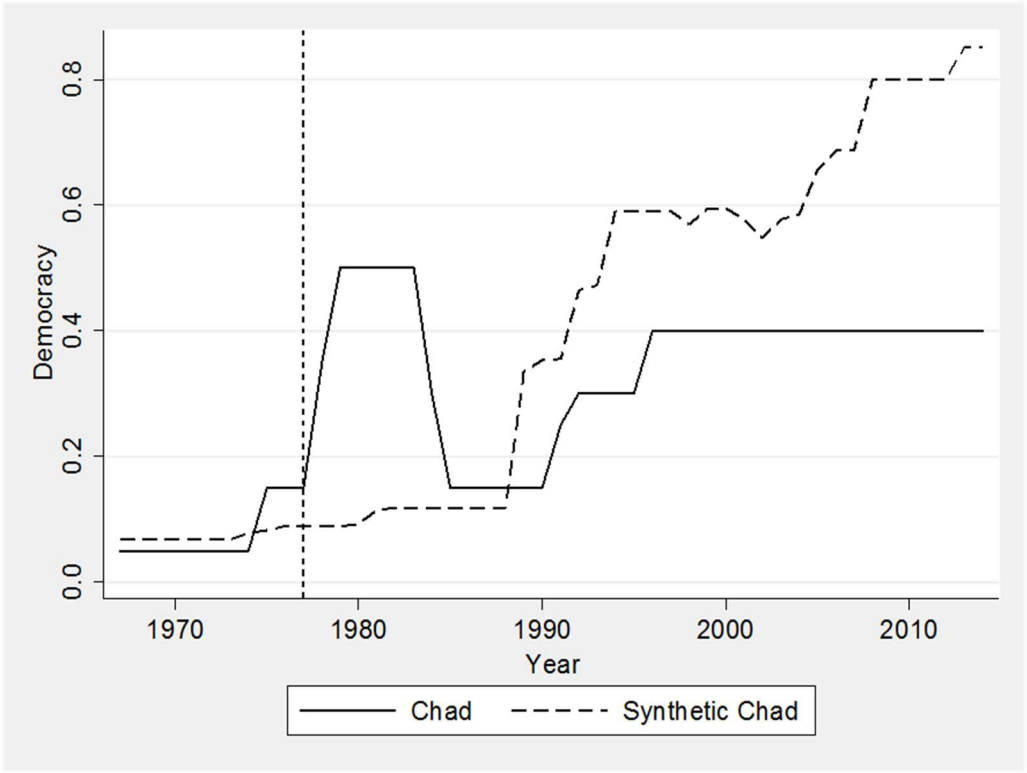


Figure 2.3 - Path of democracy: (a) Chad versus synthetic control; (b) outcome gap

(a)



(b)

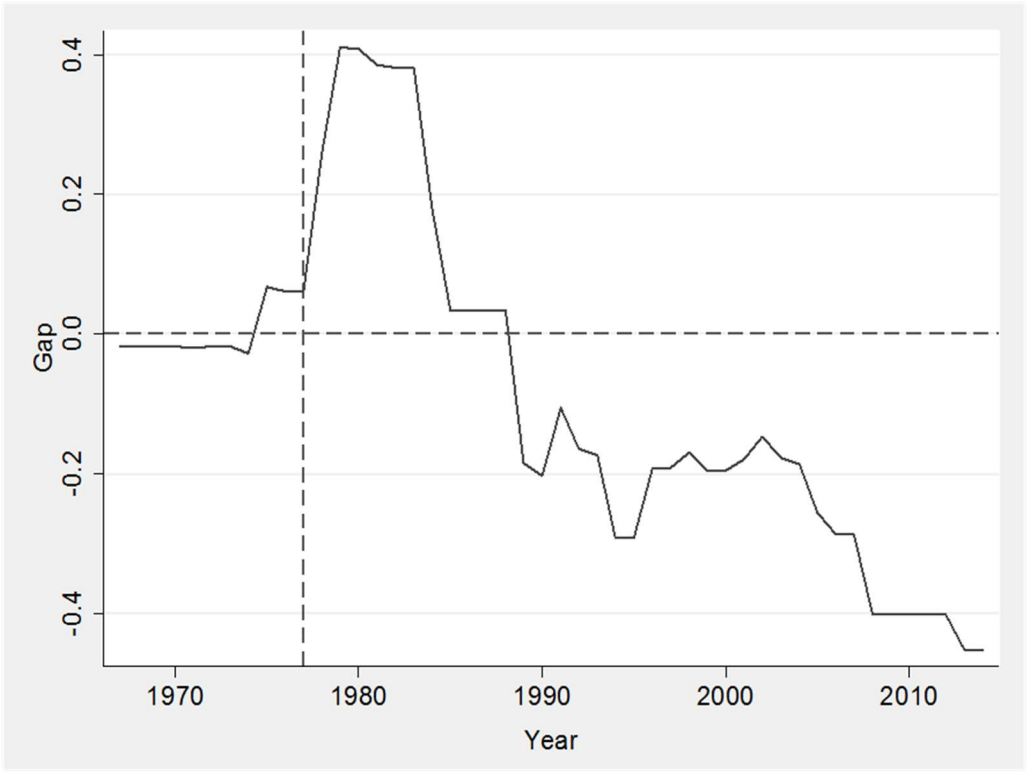
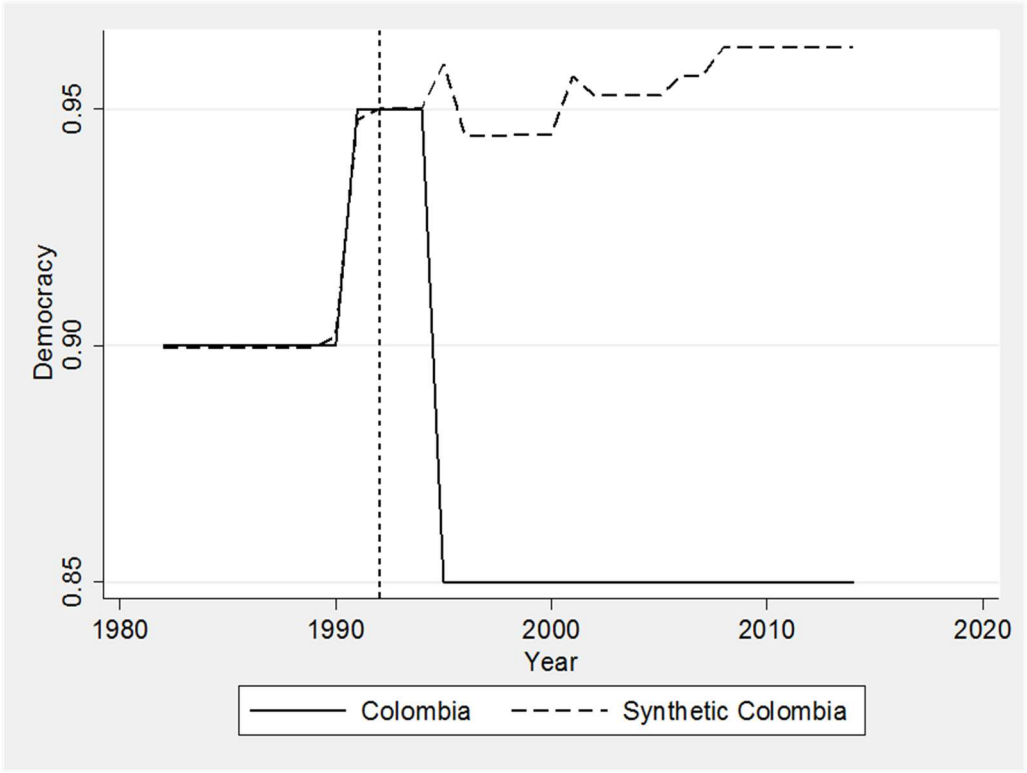


Figure 2.4 - Path of democracy: (a) Colombia versus synthetic control; (b) outcome gap

(a)



(b)

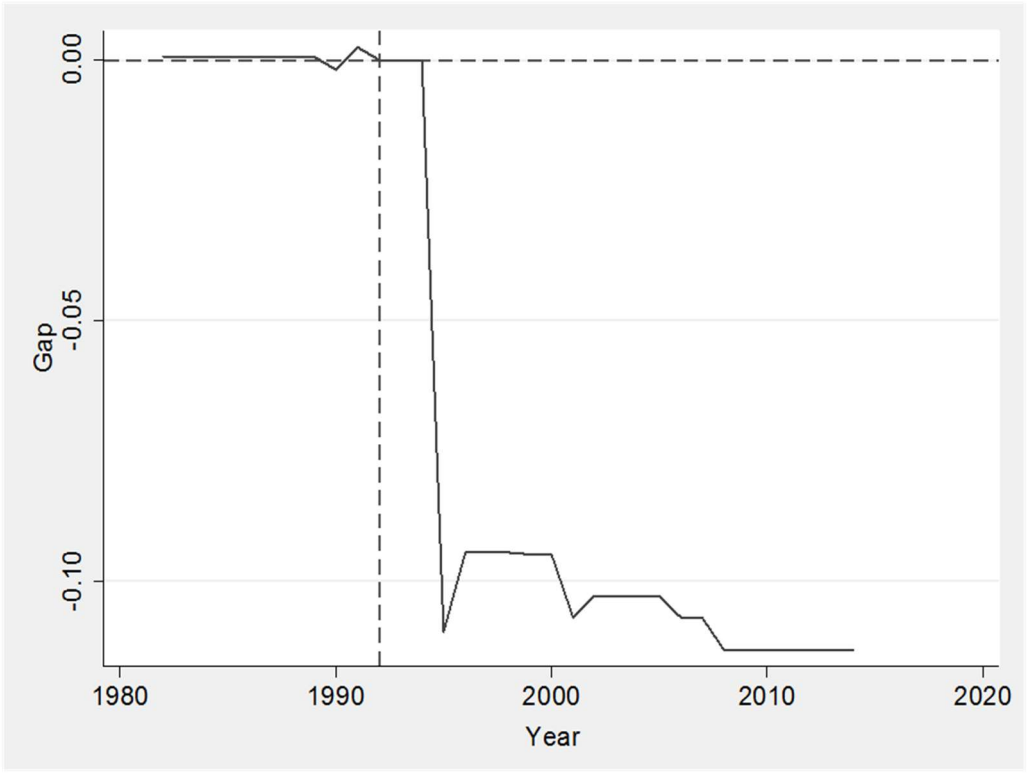
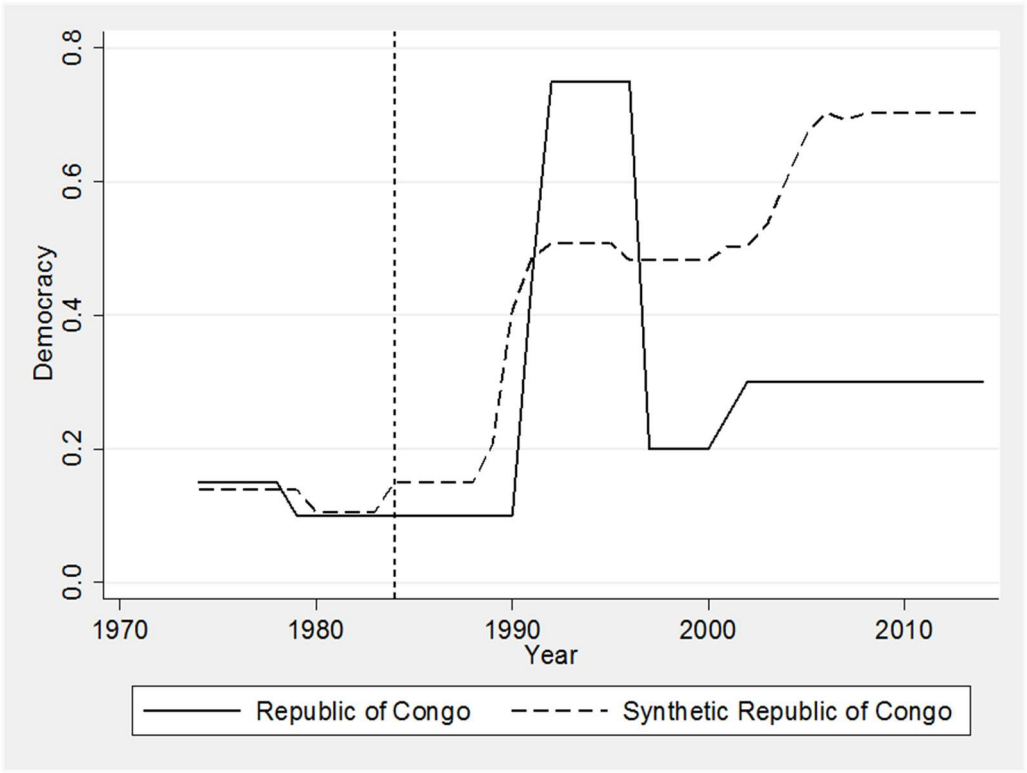


Figure 2.5 - Path of democracy: (a) Republic of Congo versus synthetic control; (b) outcome gap

(a)



(b)

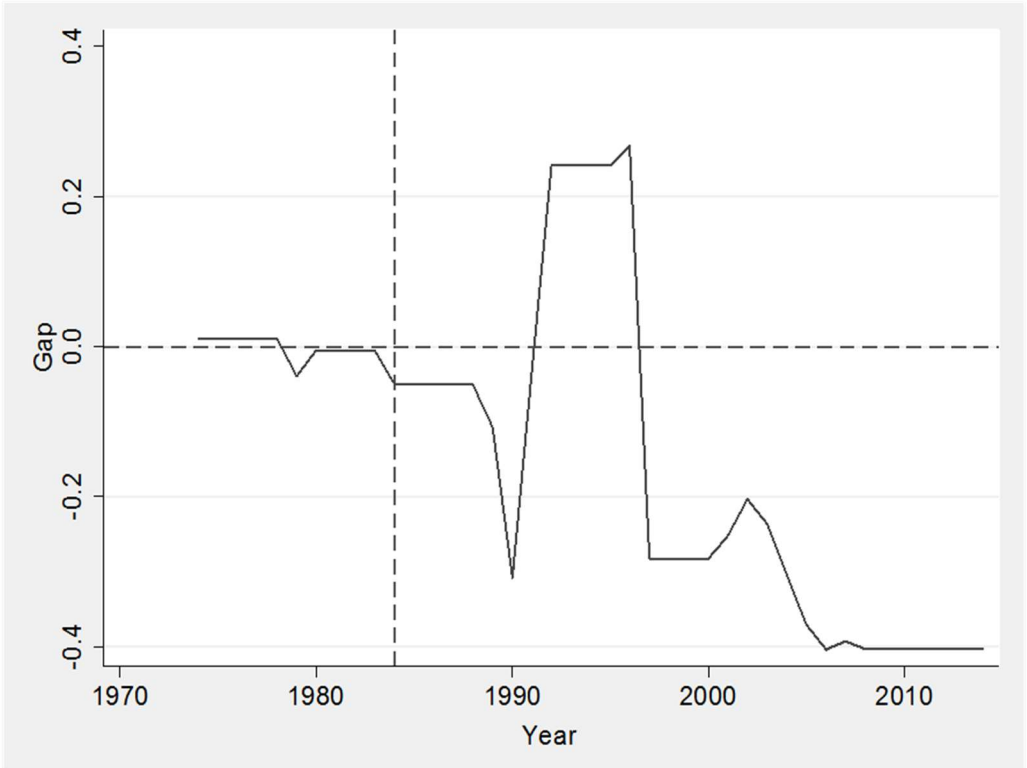
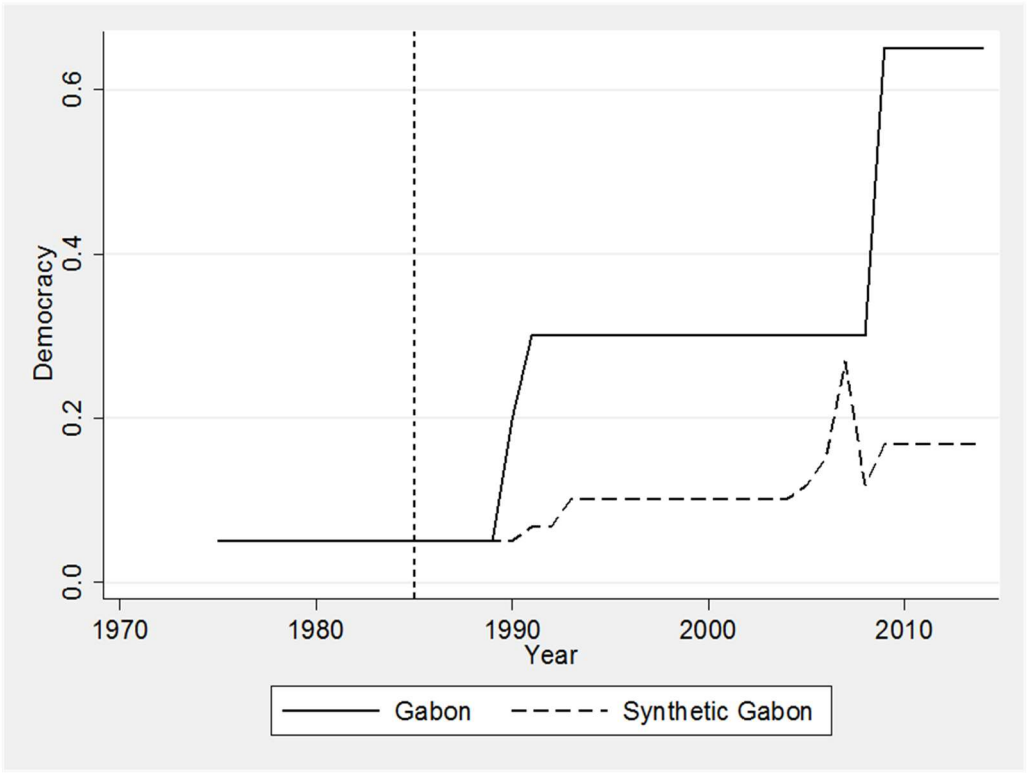


Figure 2.6 - Path of democracy: (a) Gabon versus synthetic control; (b) outcome gap

(a)



(b)

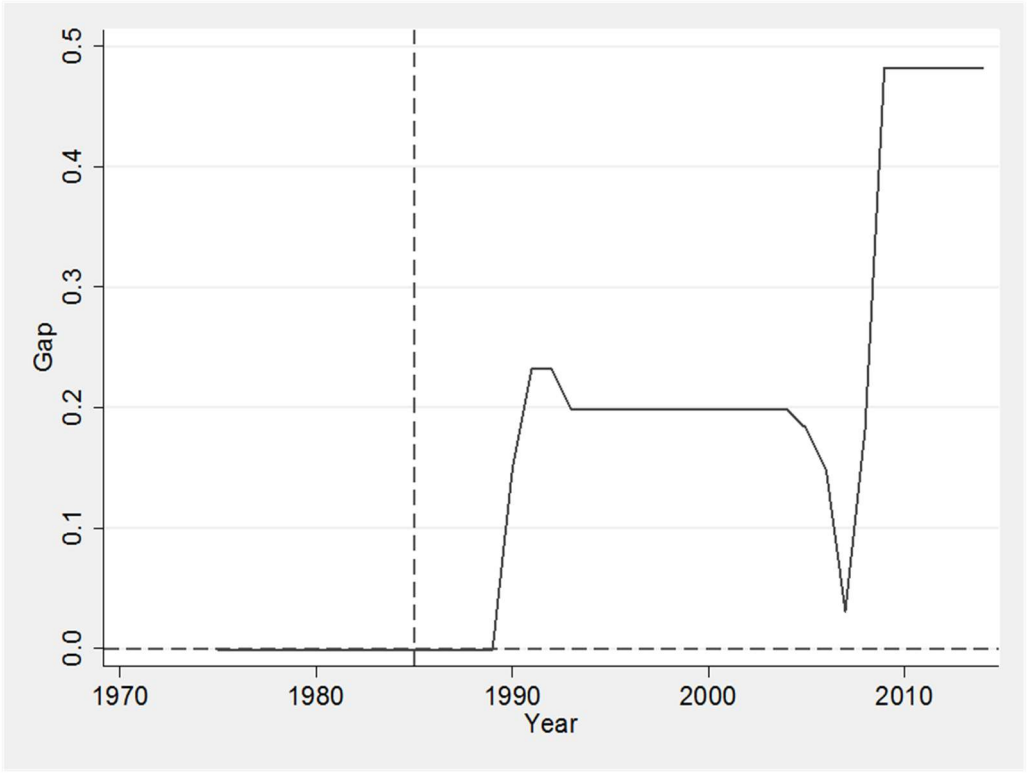
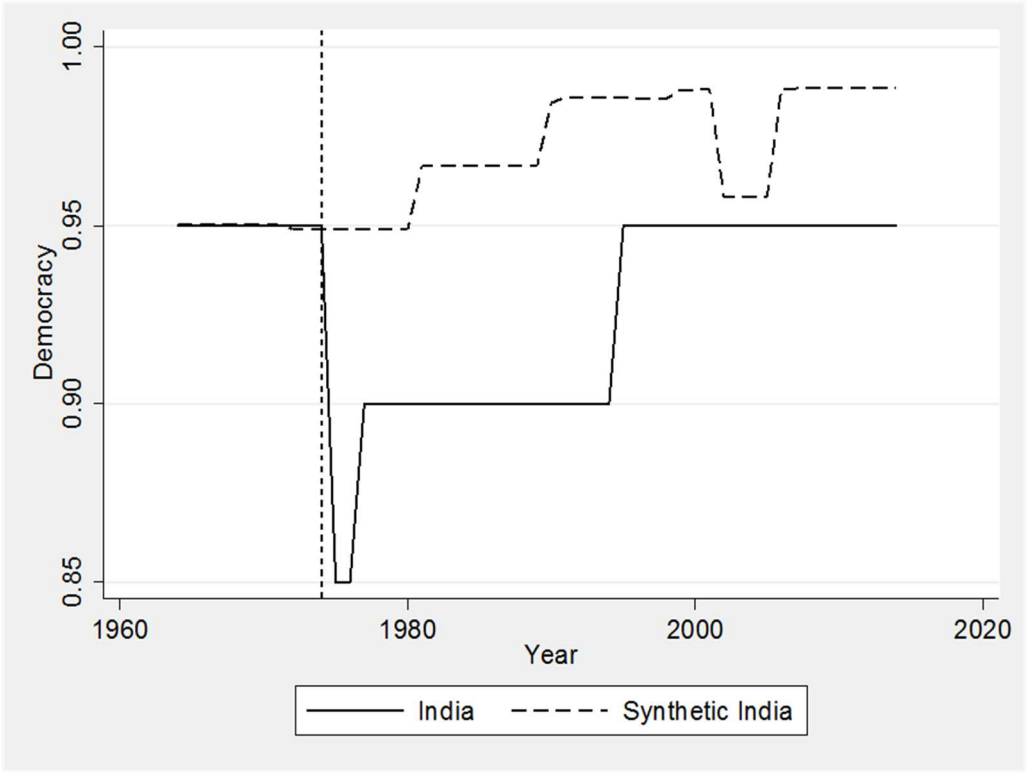


Figure 2.7 - Path of democracy: (a) India versus synthetic control; (b) outcome gap

(a)



(b)

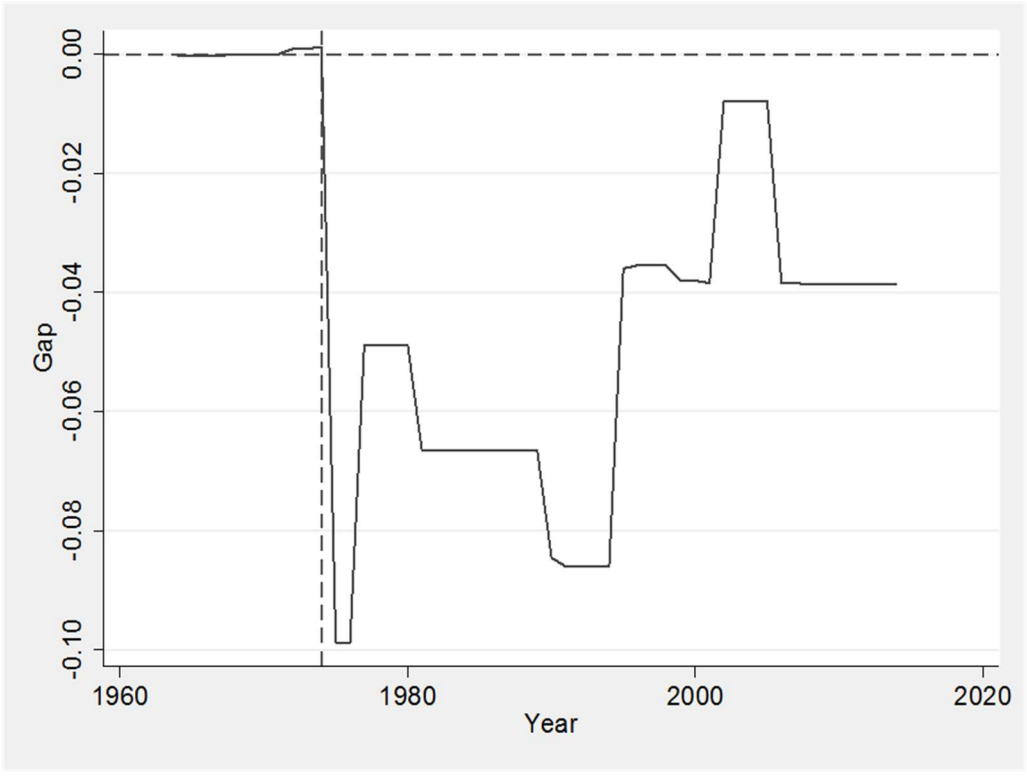
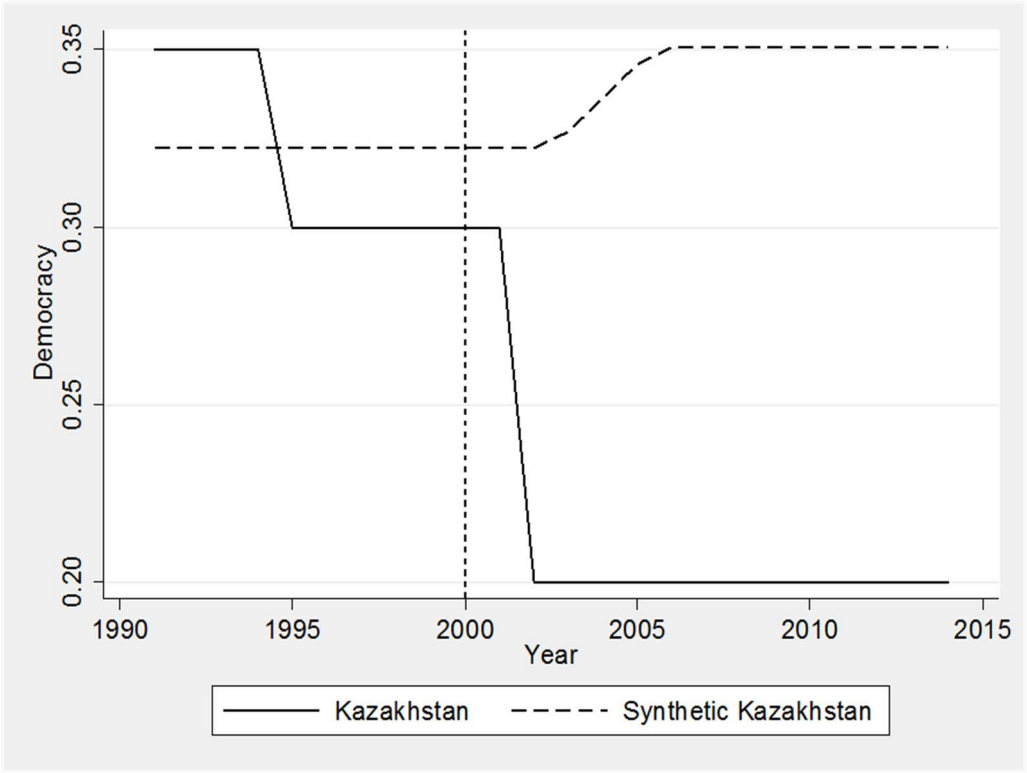


Figure 2.8 - Path of democracy: (a) Kazakhstan versus synthetic control; (b) outcome gap

(a)



(b)

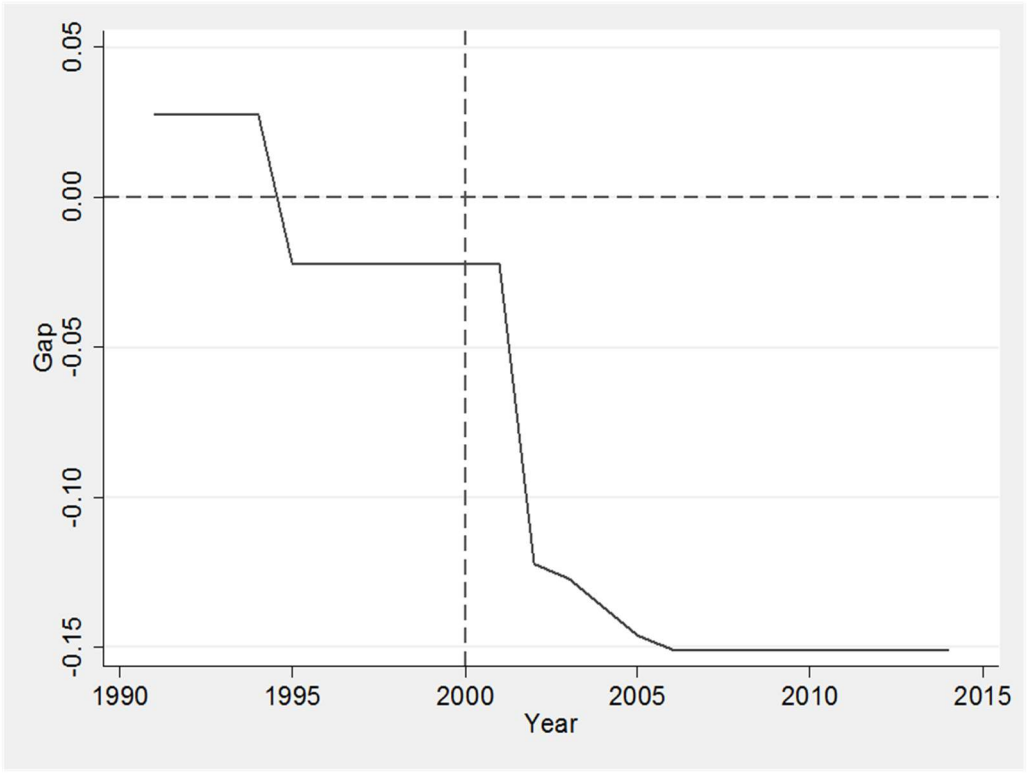
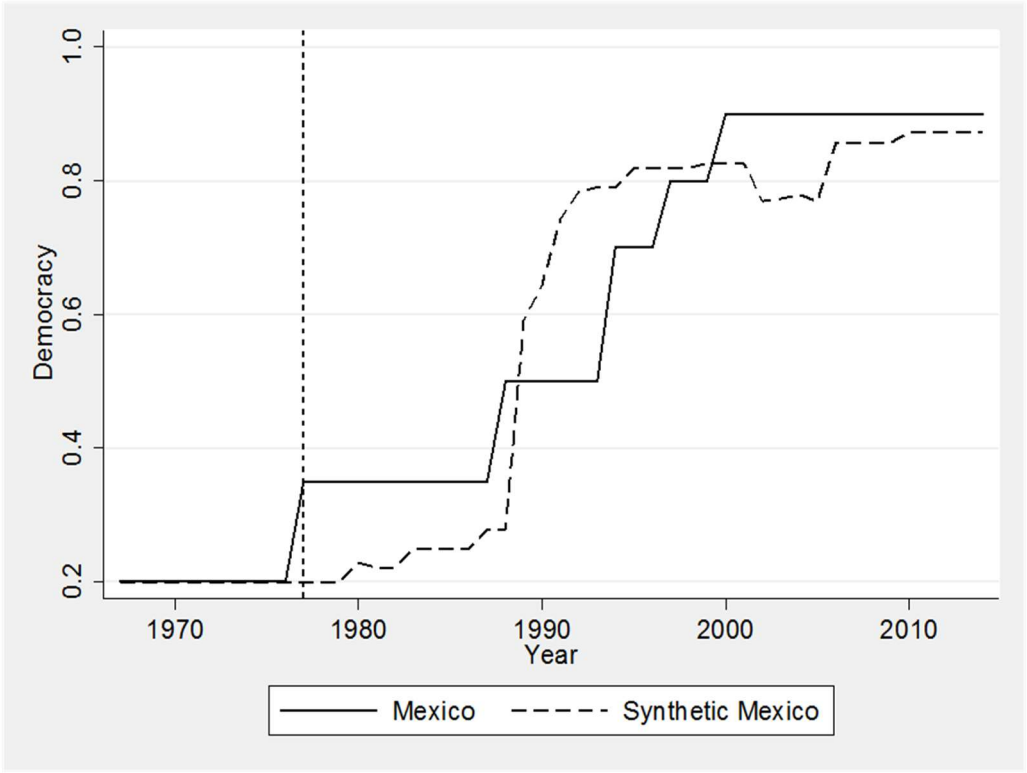


Figure 2.9 - Path of democracy: (a) Mexico versus synthetic control; (b) outcome gap

(a)



(b)

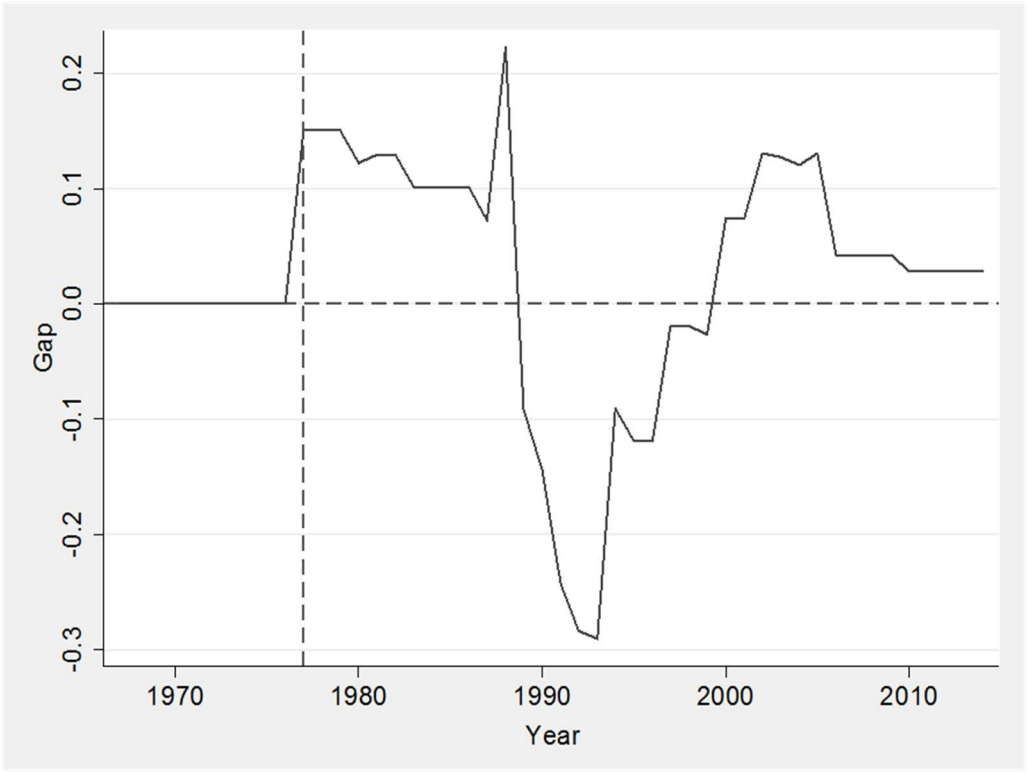
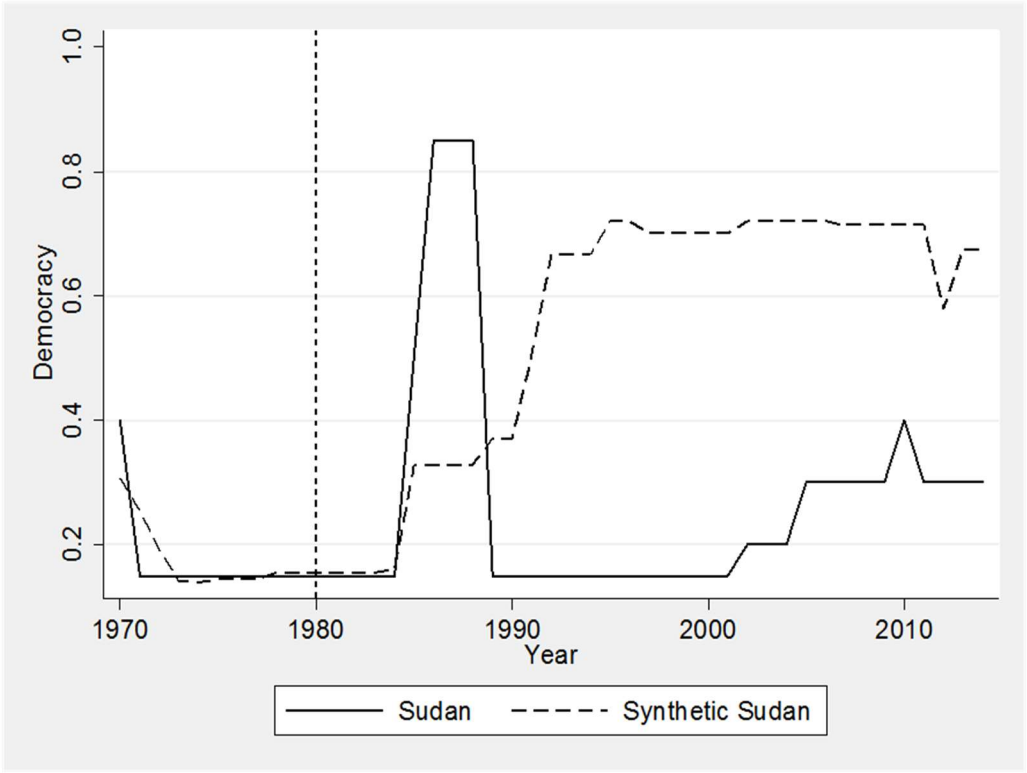


Figure 2.10 - Path of democracy: (a) Sudan versus synthetic control; (b) outcome gap

(a)



(b)

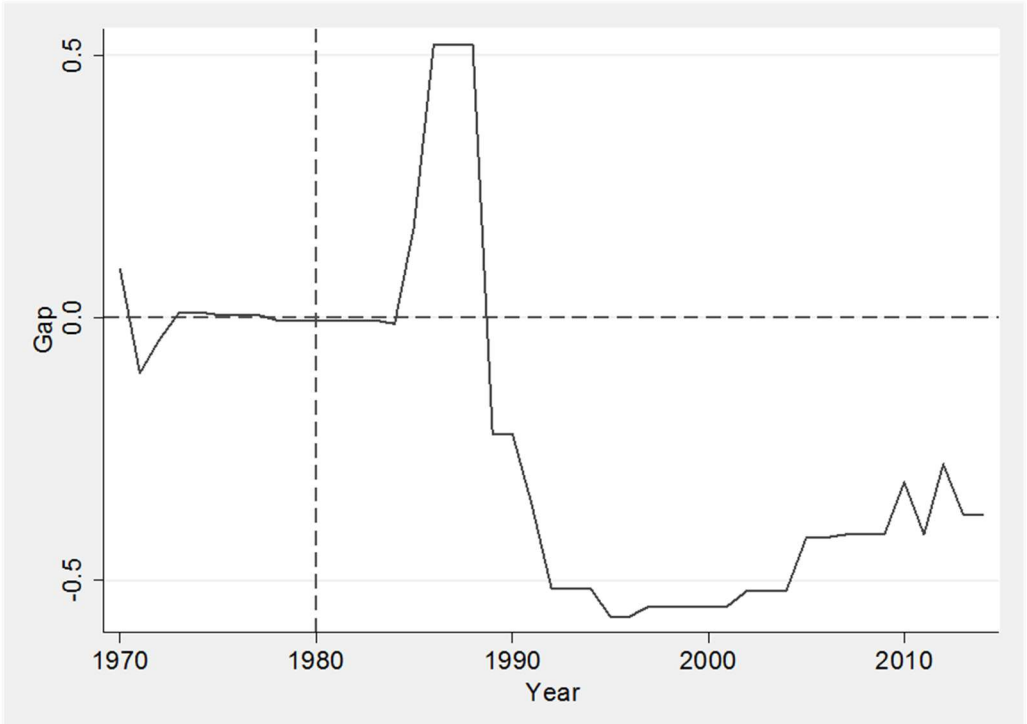
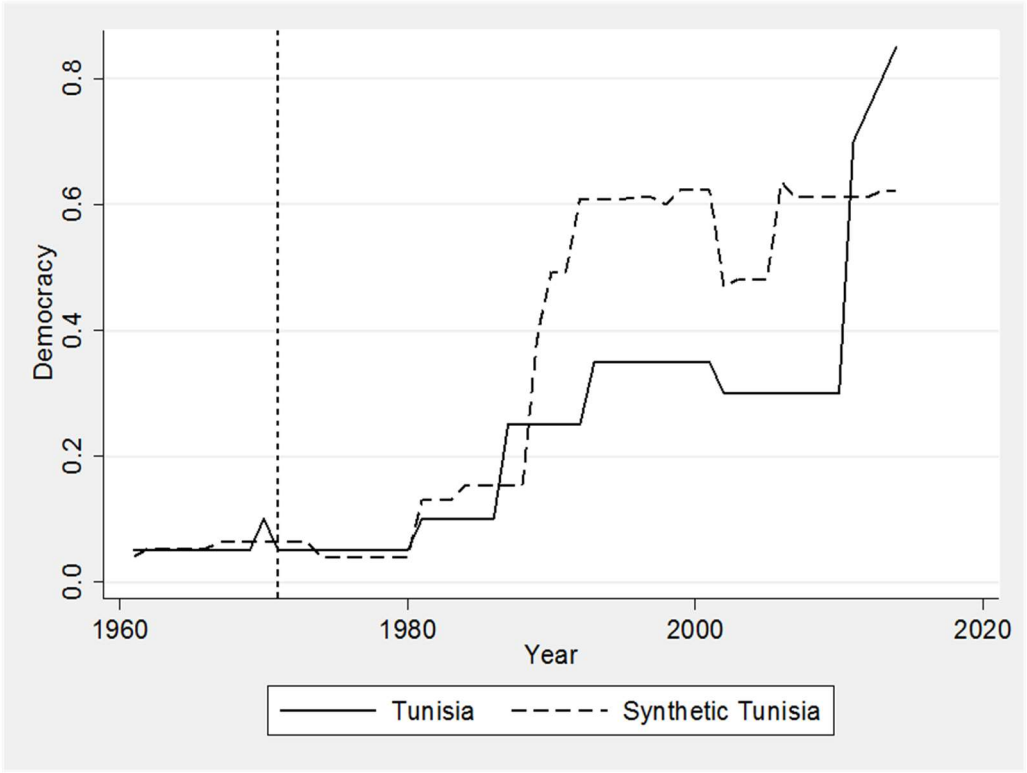


Figure 2.11 - Path of democracy: (a) Tunisia versus synthetic control; (b) outcome gap

(a)



(b)

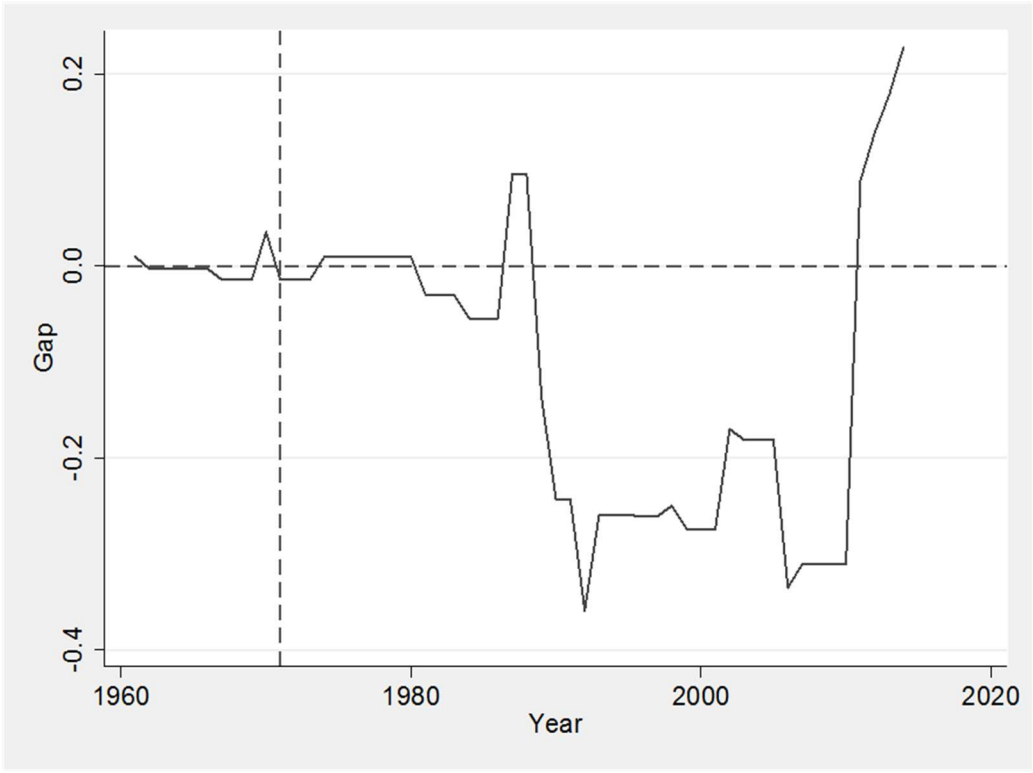
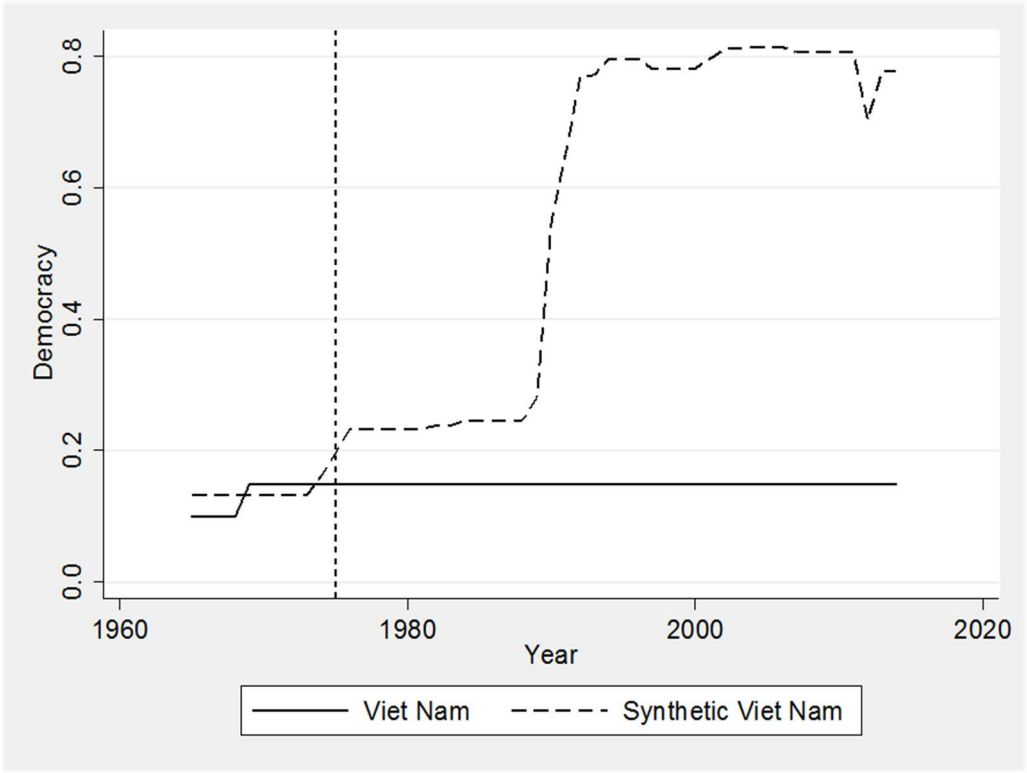


Figure 2.12 - Path of democracy: (a) Viet Nam versus synthetic control; (b) outcome gap

(a)



(b)

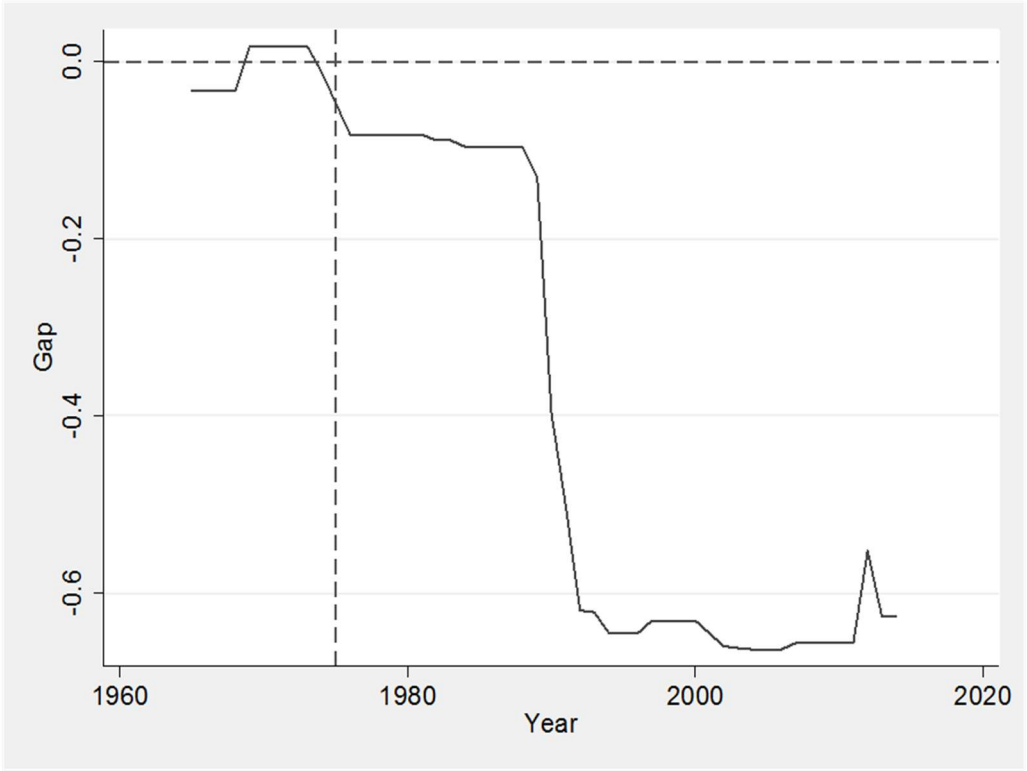
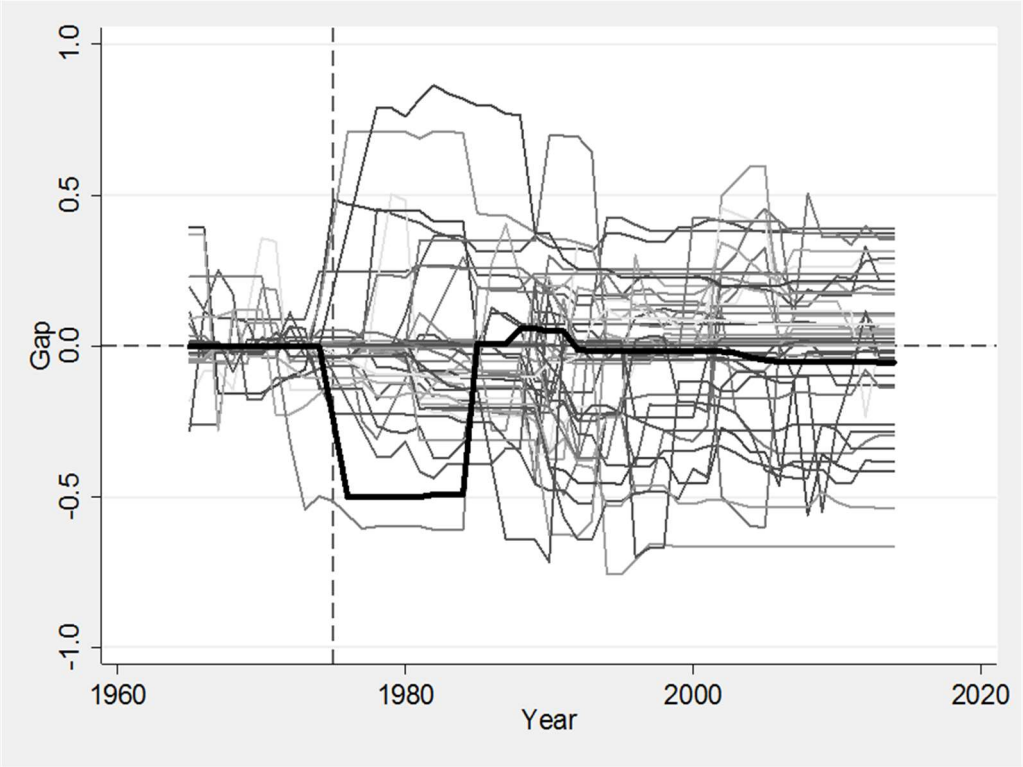
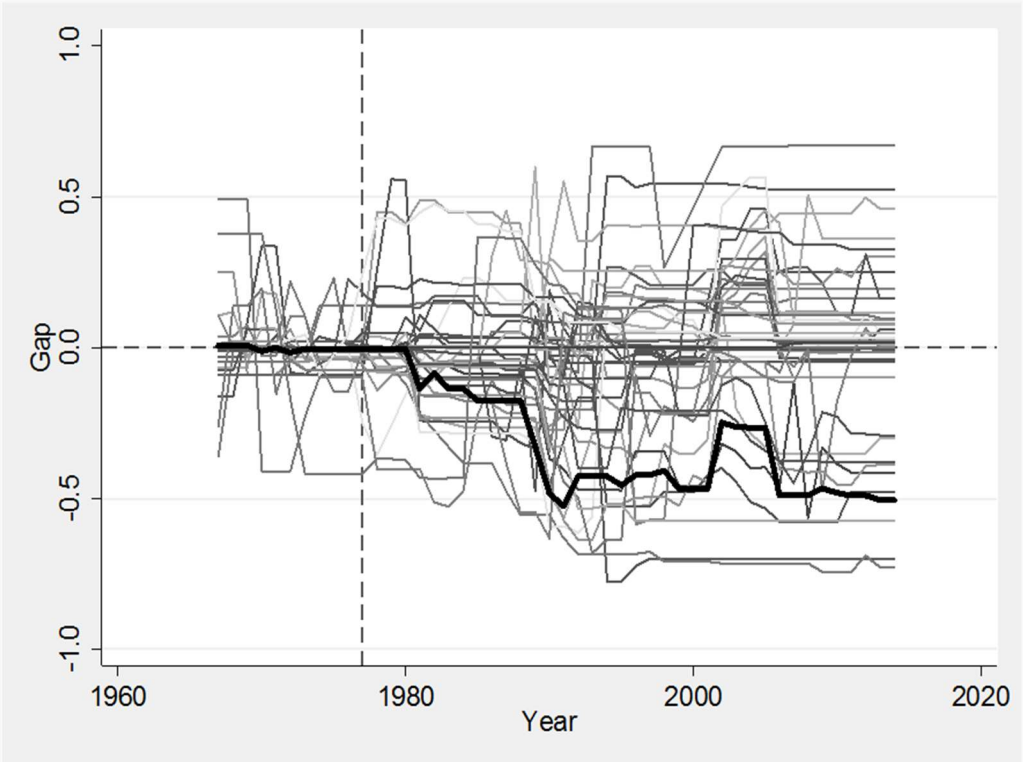


Figure 2.13 - (a)–(l) Placebo tests

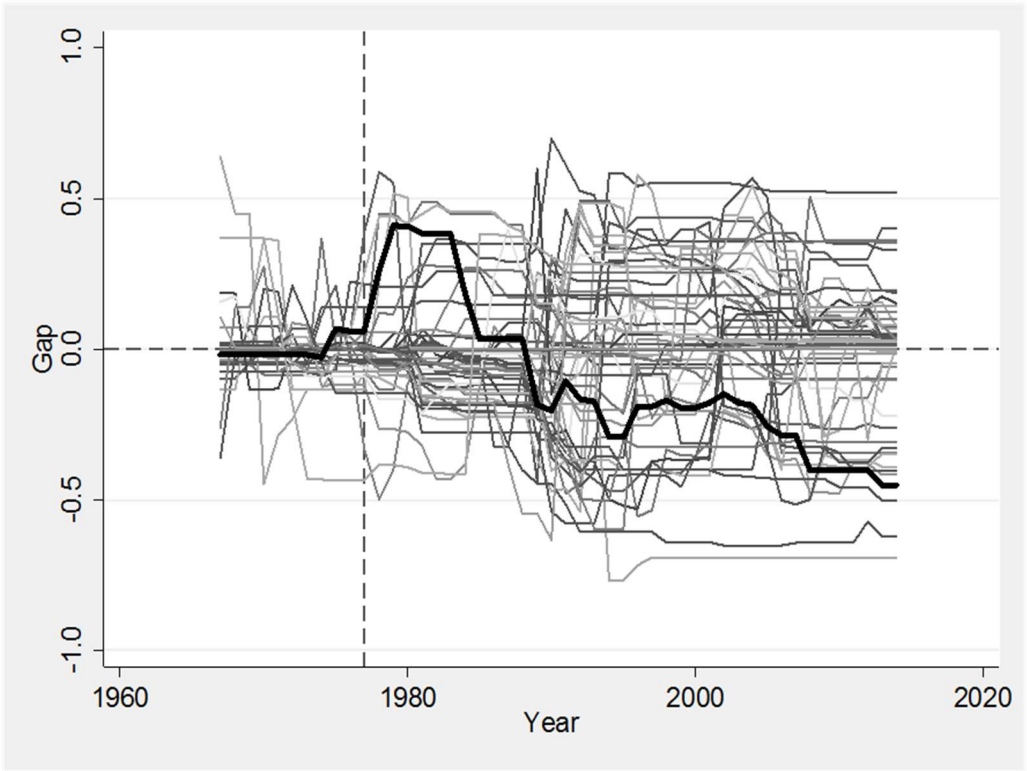
(a) Brazil



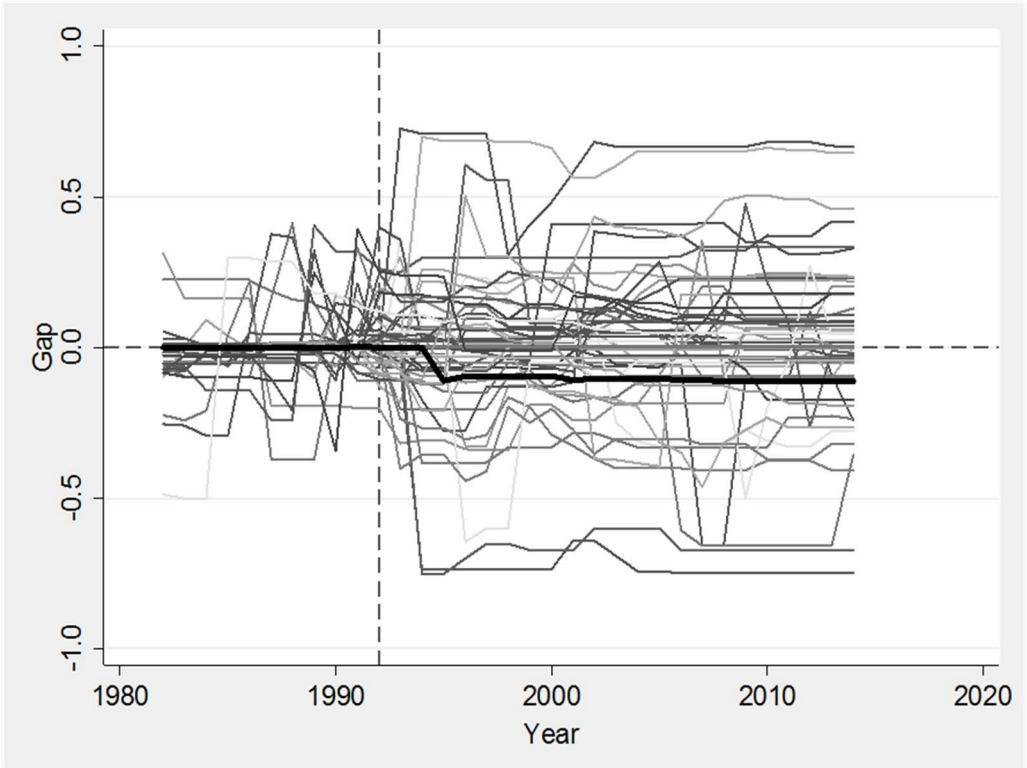
(b) Cameroon



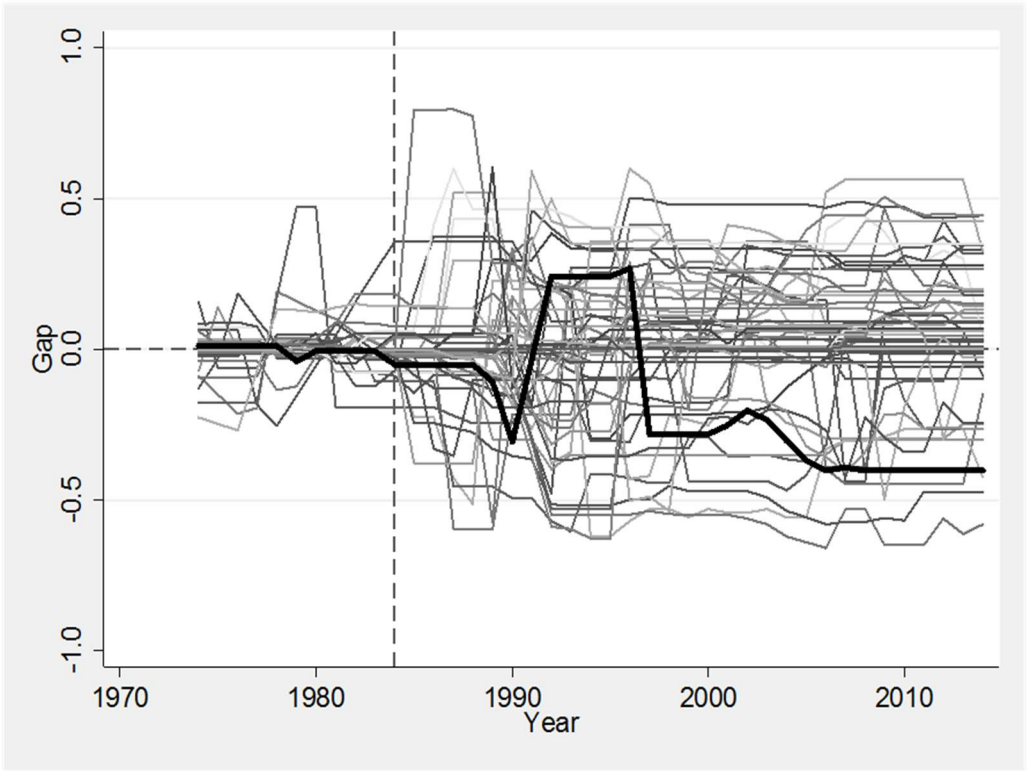
(c) Chad



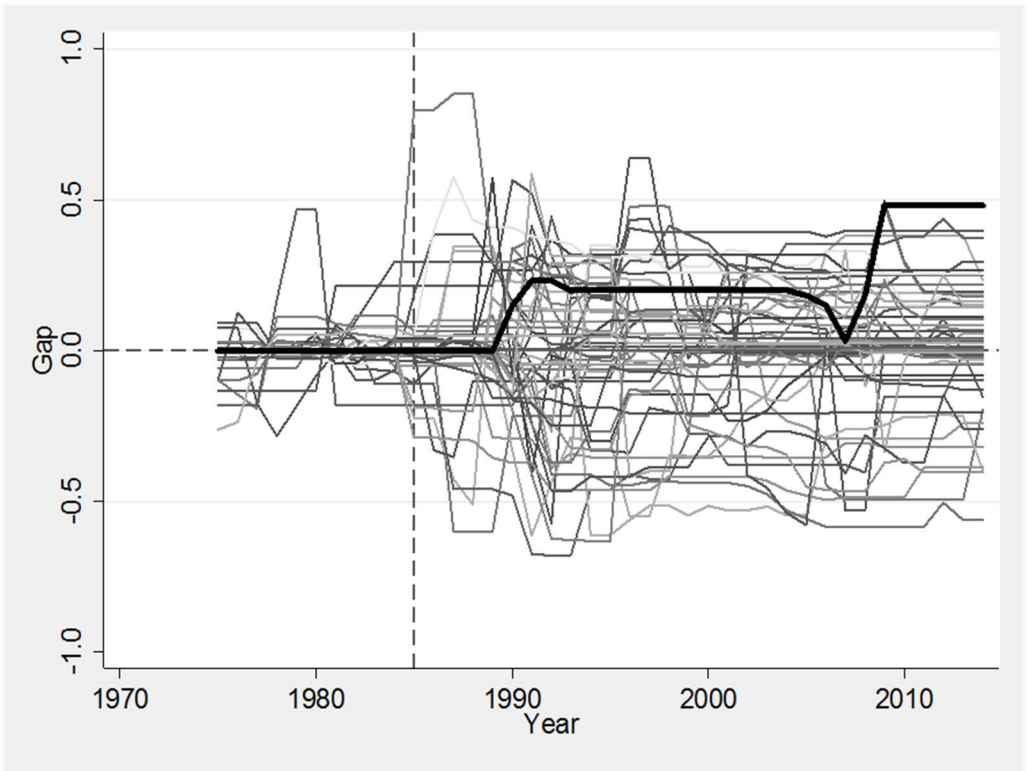
(d) Colombia



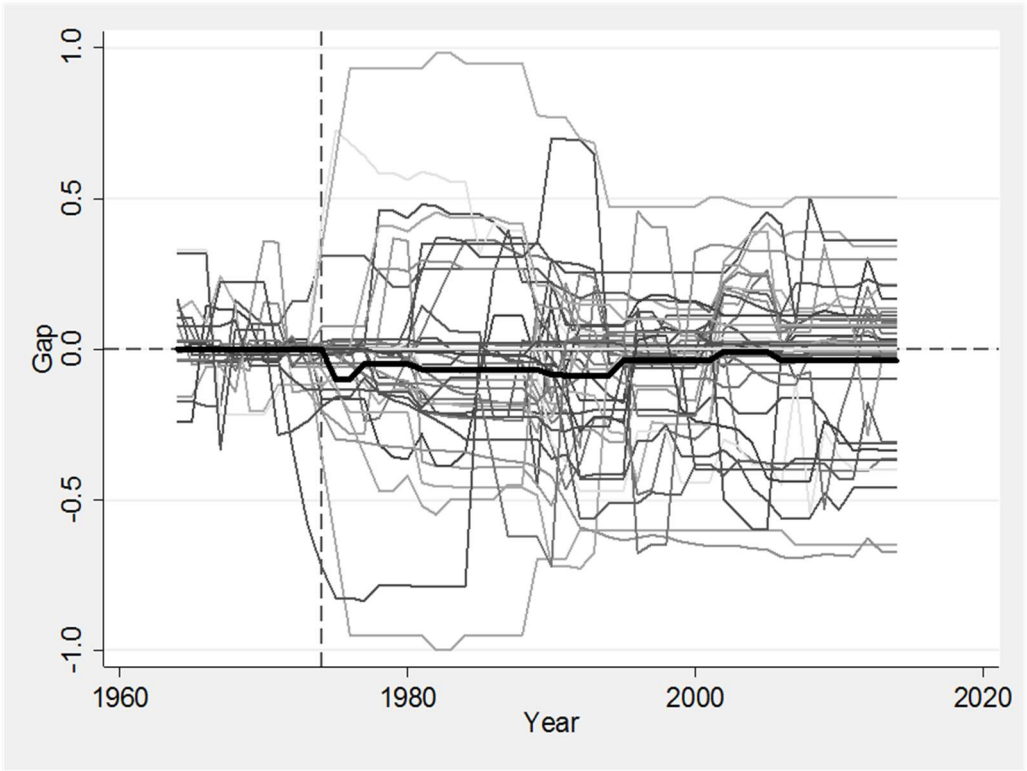
(e) Republic of Congo



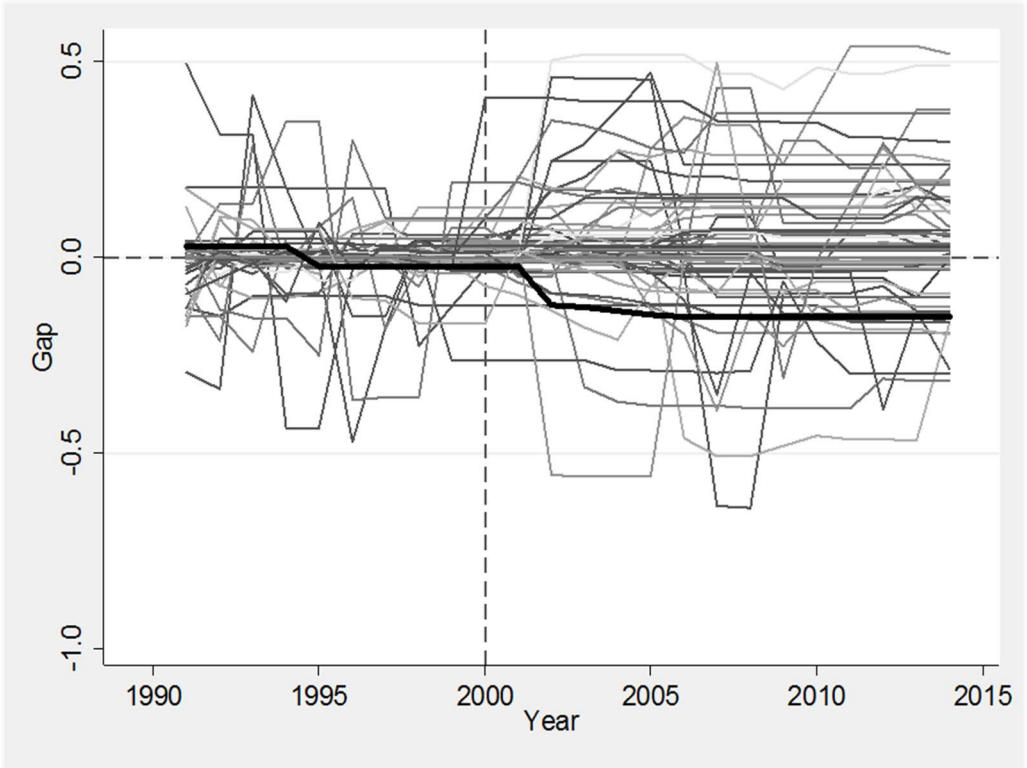
(f) Gabon



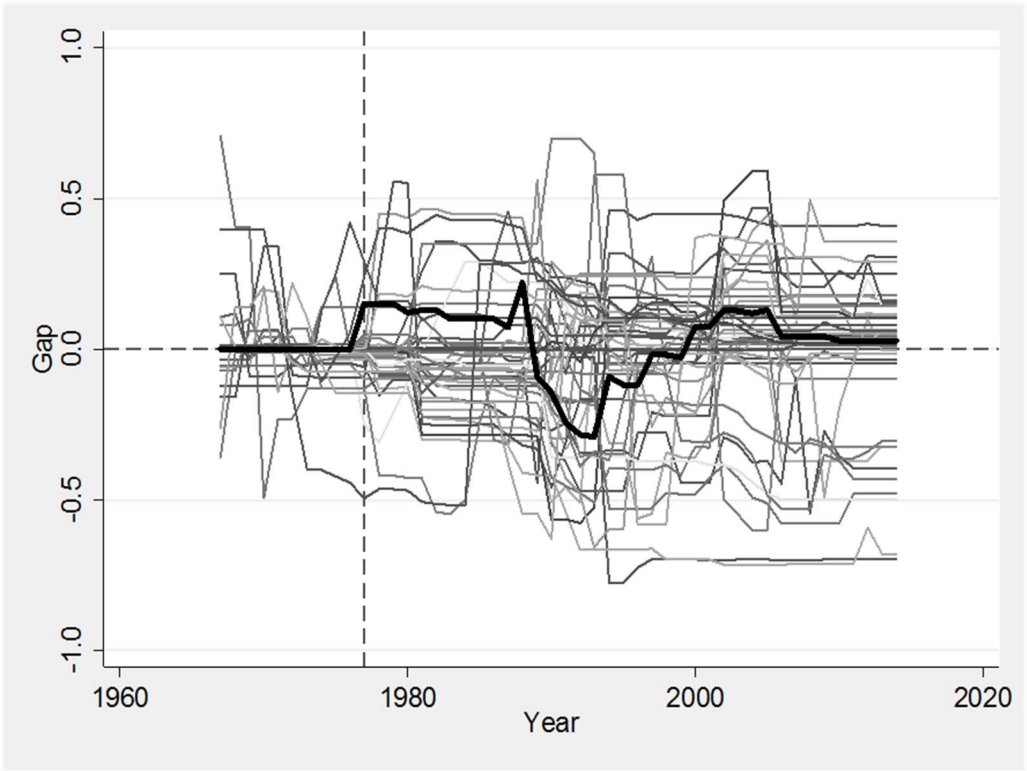
(g) India



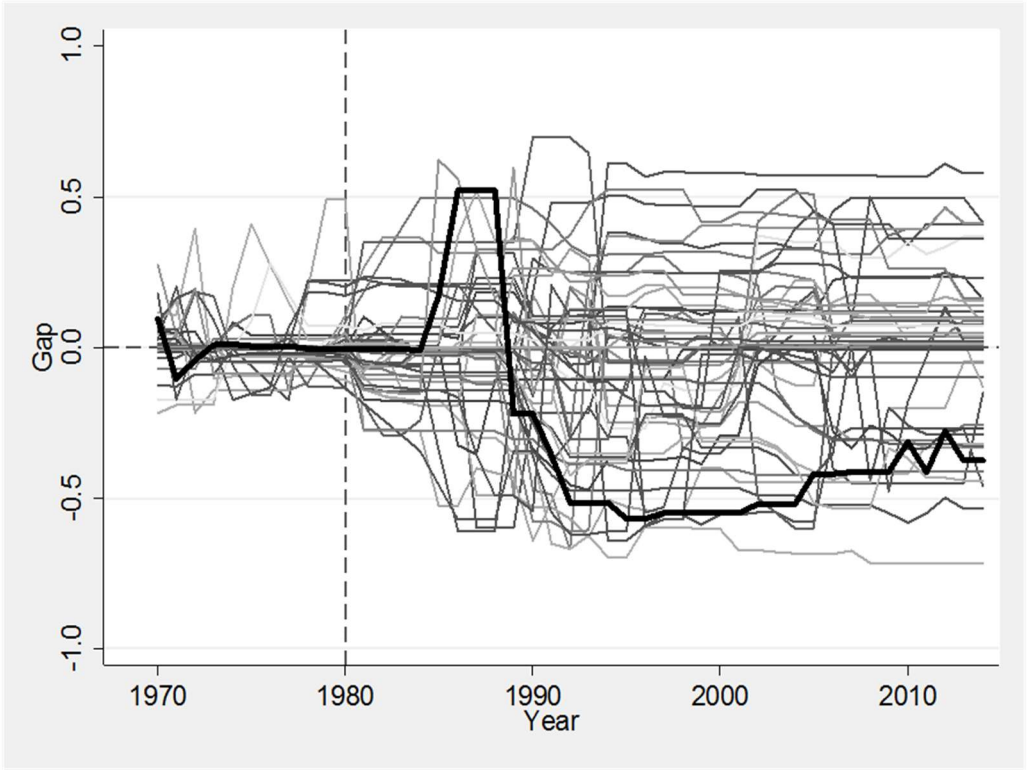
(h) Kazakhstan



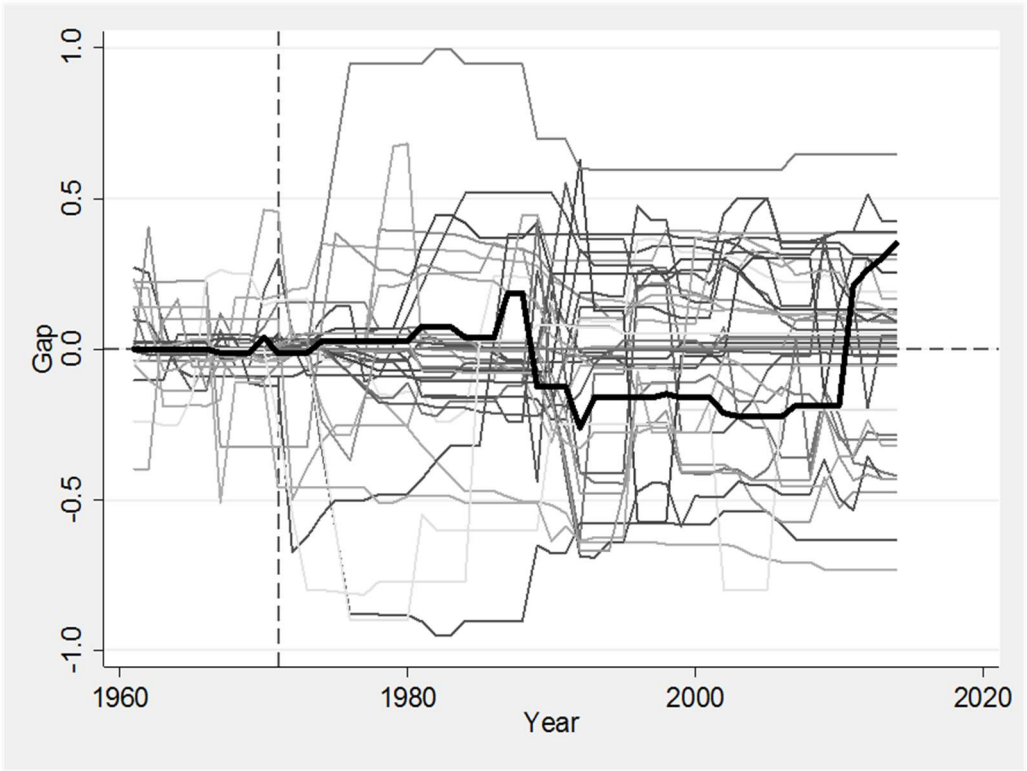
(i) Mexico



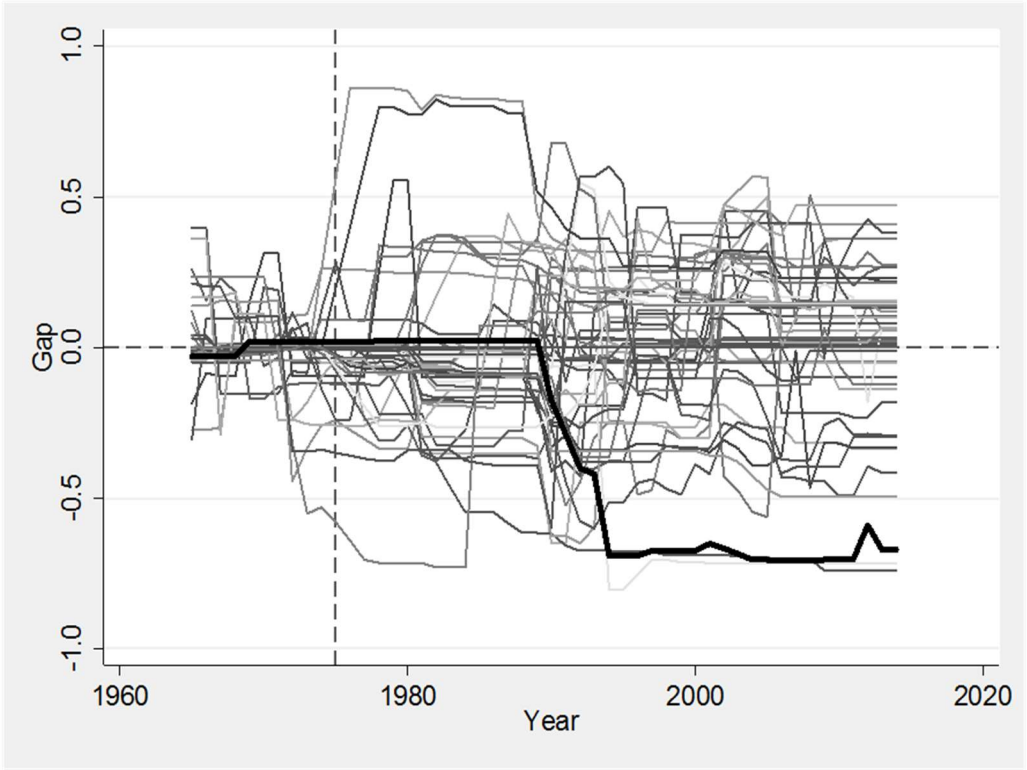
(j) Sudan



(k) Tunisia



(l) Viet Nam



Appendix

Table 2.A.1 - Discoveries excluded from the case studies

Country	Peak of oil discoveries	Country	Peak of oil discoveries
Albania	1928	Italy	1981
Algeria	1956	Kuwait	1938
Angola	1971	Libya	1961
Argentina	1960	Nigeria	1967
Australia	1967	Oman	1962
Austria	1947	Peru	1861
Azerbaijan	1871	Qatar	1940
Bahrain	1932	Romania	1857
Bolivia	1966	Russia	1960
Canada	1958	Saudi Arabia	1948
Chile	1960	Syria	1966
China	1959	Trinidad	1959
Croatia	1950	Turkey	1969
Ecuador	1969	Turkmenistan	1964
Egypt	1965	Ukraine	1962
France	1958	United Arab Emirates	1980
Germany	1952	United States	1930
Hungary	1964	Uzbekistan	1992
Indonesia	1945	Venezuela	1941
Iran	1961	Yemen	1978

Source: Campbell (2006).

Table 2.A.2 - Country weights in the synthetic control and potential controls

Brazil	
Synthetic control	Central African Republic (0.01), Democratic Republic of the Congo (0.153), Morocco (0.004), Portugal (0.833)
Potential controls	Belgium, Bulgaria, Burundi, Sri Lanka, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zambia
Cameroon	
Synthetic control	Benin (0.075), Nepal (0.368), Niger (0.049), Paraguay (0.301), Tanzania (0.156), Uruguay (0.051)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic, Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Morocco, New Zealand, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Togo, Zambia
Chad	
Synthetic control	Bhutan (0.286), Ethiopia (0.007), Honduras (0.027), Malawi (0.169), Nepal (0.052), Paraguay (0.434), Portugal (0.023)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Guinea, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic, Lesotho, Liberia, Luxembourg, Madagascar, Mali, Mauritania, Mongolia, Morocco, New Zealand, Niger, Philippines, Poland, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Burkina Faso, Uruguay, Zambia
Colombia	
Synthetic control	Nepal (0.007), Spain (0.884), Tanzania (0.048), Zambia (0.061)
Potential controls	Bangladesh, Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Czech Republic, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic, Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mauritius, Mongolia, Morocco, Mozambique, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Swaziland, Sweden, Switzerland, Togo, Uganda, Uruguay, Zimbabwe
Republic of Congo	
Synthetic control	Jordan (0.228), Liberia (0.669), Zambia (0.103)
Potential controls	Bangladesh, Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Kenya, Republic of Korea, Lao People's Democratic Republic, Lesotho, Luxembourg, Malawi, Mali, Mauritania, Mauritius, Mongolia, Morocco, Nepal, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Swaziland, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zimbabwe
Gabon	
Synthetic control	Mauritania (0.336), Singapore (0.001), Swaziland (0.664)
Potential controls	Bangladesh, Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic, Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritius, Mongolia, Morocco, Nepal, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, South Africa, Zimbabwe, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zambia

Continued on next page

… table 2.A.2 continued

India	
Synthetic control	Costa Rica (0.429), Japan (0.517), Lao People's Democratic Republic (0.001), Nepal (0.051), Zambia (0.002)
Potential controls	Belgium, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Jordan, Kenya, Republic of Korea, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Morocco, New Zealand, Niger, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay
Kazakhstan	
Synthetic control	Lao People's Democratic Republic (0.349), Liberia (0.095), Singapore (0.556)
Potential controls	Bangladesh, Armenia, Belgium, Botswana, Bulgaria, Burundi, Cambodia, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Czech Republic, Benin, Dominican Republic, El Salvador, Estonia, Fiji, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Kenya, Republic of Korea, Kyrgyzstan, Lesotho, Latvia, Lithuania, Luxembourg, Malawi, Mali, Mauritania, Mauritius, Mongolia, Moldova, Morocco, Mozambique, Namibia, Nepal, New Zealand, Niger, Panama, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Slovenia, South Africa, Spain, Swaziland, Sweden, Switzerland, Tajikistan, Togo, Uganda, Tanzania, Uruguay, Zambia, Zimbabwe
Mexico	
Synthetic control	Democratic Republic of the Congo (0.067), Japan (0.083), Nepal (0.142), Poland (0.57), Togo (0.137)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Jordan, Kenya, Republic of Korea, Lao People's Democratic Republic, Lesotho, Liberia, Luxembourg, Malawi, Mali, Mauritania, Mongolia, Morocco, New Zealand, Niger, Paraguay, Philippines, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sweden, Switzerland, Tanzania, Uruguay, Zambia
Sudan	
Synthetic control	Greece (0.014), Jordan (0.126), Mali (0.385), Tanzania (0.267), Uruguay (0.208)
Potential controls	Belgium, Botswana, Bulgaria, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Fiji, Finland, Gambia, Ghana, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Kenya, Republic of Korea, Lao People's Democratic Republic, Lesotho, Liberia, Luxembourg, Malawi, Mauritania, Mauritius, Mongolia, Morocco, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, Singapore, South Africa, Spain, Swaziland, Sweden, Switzerland, Togo, Zambia, Zimbabwe
Tunisia	
Synthetic control	Jordan (0.485), Mongolia (0.034), Nepal (0.257), Paraguay (0.224)
Potential controls	Belgium, Bulgaria, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Jordan, Republic of Korea, Lao, Liberia, Luxembourg, Mali, Mauritania, Mongolia, Morocco, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Portugal, Rwanda, Senegal, Sierra Leone, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay
Viet Nam	
Synthetic control	Bulgaria (0.355), Jordan (0.143), Malawi (0.035), Mali (0.297), Portugal (0.119), Singapore (0.05)
Potential controls	Belgium, Burundi, Central African Republic, Sri Lanka, Democratic Republic of the Congo, Costa Rica, Cyprus, Benin, Dominican Republic, El Salvador, Finland, Gambia, Ghana, Greece, Guatemala, Honduras, Ireland, Israel, Jamaica, Japan, Kenya, Republic of Korea, Lao People's Democratic Republic, Liberia, Luxembourg, Mauritania, Mongolia, Morocco, Nepal, New Zealand, Niger, Paraguay, Philippines, Poland, Rwanda, Senegal, Sierra Leone, South Africa, Spain, Sweden, Switzerland, Togo, Tanzania, Uruguay, Zambia

3. Is there a fiscal resource curse? The mitigation effect of political institutions

Abstract

While several studies have focused on the effect of natural resources on economic development, less attention has been paid to their effects on other development outcomes. We contribute to this literature by studying the impact of resource rents on tax systems. We posit that natural resource rents reduce the incentives to invest in fiscal capacity, which is the ability to raise revenues from broad tax bases. However, political institutions that limit the power of the executive may mitigate or neutralise the negative effect of natural resources on fiscal infrastructures. We provide empirical support to this hypothesis using panel data covering the period 1981-2011 and 100 developing countries. Moreover, we show that the effect of natural resources is likely to work through fiscal institutions that make the state accountable to, and transparent for, its citizens.

Keywords: state capacity, fiscal capacity, resource curse, institutions, economic development

JEL Codes: O4, P5, N4

This essay is based on joint work with A. Savoia and K. Sen (both at the University of Manchester)

3.1 Introduction

The effect of natural resource abundance on the economy has been a lively area of research for many years. Traditionally, most research has concentrated on long-term growth effects, initially finding a "resource curse", and more recently arguing that the long-term effect of specialising in natural resources depends on the type of resources (e.g., Isham et al. 2005) and the quality of the institutional environment in the economy (e.g., Mehlum et al. 2006)[1]. As yet, less analysis has been devoted to other development outcomes. For example, underexplored areas include the effects on inequality (Carmignani 2013; Goderis and Malone 2011; Fum and Hodler 2010), education (Ebeke et al 2015; Stijns 2006), health and living standards (Edwards 2016; Pineda and Rodriguez 2010; Caselli and Michaels 2013). This paper contributes to the literature by looking at a further underexplored issue: the effects of natural resource abundance on the state and, in particular, fiscal capacity[2]. We provide a systematic econometric analysis of the effect of resource abundance on fiscal systems, arguing that it appears to depend on the quality of political institutions.

Our hypothesis is that natural resource rents reduce the incentives to invest in fiscal capacity, but political institutions limiting the power of the executive and

[1] Many studies have addressed the counter-intuitive idea that countries that are highly endowed in exploitable natural resources perform worse than those without. Much of the early literature argues the adverse effect of natural resource wealth on economic growth (e.g., Sachs and Warner, 1995, 1999, 2001; Rodriguez and Sachs, 1999; Gylfason, 2001). See van der Ploeg (2011) for a comprehensive survey of the hypotheses and evidence. Alongside the focus on growth, the literature has also shown that natural resources abundance leads to higher level of corruption (e.g., Caselli and Michaels, 2013), civil conflicts (e.g., Collier and Hoeffer, 2004), and less democracy (e.g. Ross, 2001). The negative effects of natural resources are, however, controversial. For example, Alexeev and Conrad (2009) claim that a large endowment of oil and mineral resources has a positive effect on long-term economic growth and does not negatively impact on the quality of institutions. Cotet and Tsui (2013) contradict the statistical association between the value of oil reserves and the onset of civil war, and Haber and Menaldo (2011) find that increasing resource reliance does not promote dictatorship over the long run. Finally, Stijns (2006) denies the negative correlation between resource abundance and human capital.

[2] Following Besley and Persson (2011), we consider fiscal capacity the ability of a fiscal system to raise revenues from a broad tax base.

hence promoting accountability and common interests, may mitigate or neutralise the negative effect of natural resources on fiscal infrastructures. To test this hypothesis, we use panel methods on a sample of 100 developing countries, from 1981 to 2011. Our fiscal capacity measure, the share of non-resource taxes on income, profits, and capital gains on non-resource total taxes, is based on the assumption that collecting income taxes requires a more developed and competent administrative structure than raising other types of taxes (see Besley and Persson 2014) and is constructed using the Government Revenues Dataset (GRD) (Prichard et al., 2014), with improved coverage and the distinction between resource and non-resource revenues. We find persuasive evidence that the impact of resource income, after extensive robustness checks, depends on whether political institutions place significant constraints on executive power. Hence, a fiscal resource curse does not necessarily materialise in resource-rich economies. These results are complemented with further analysis to assess how mitigation of political institutions impacts on specific aspects of tax systems. Using a recent set of indicators provided by the Public Expenditure and Financial Accountability project (PEFA, 2006), we provide initial evidence suggesting that the effect of natural resources is likely to work through fiscal institutions that make the state accountable to, and transparent for, its citizens.

In addition to contributing to the literature on the resource curse, our paper adds to the research on the determinants of state capacity, an area which has as yet seen relatively little empirical analysis (Savoia and Sen 2015), despite now being considered strategically important for economic development (Besley and Persson 2011). Indeed, the capacity to collect revenue is indispensable for the provision of public goods and investments in infrastructure, as stylised facts suggest that developing economies collect, on average, a significantly smaller share of taxes compared to advanced market economies (Besley and Persson 2014). Hence, assessing whether a geographical feature shaping the structure of the economy, such as natural resource abundance, comes with the likely price of underdeveloped fiscal institutions may have relevant policy trade-offs. We find that this is not the case, if countries have suitable political institutions.

The paper is structured as follows: Section 2 reviews the literature and sets out our hypotheses; Section 3 describes the empirical strategy and data. In section 4, we test our hypotheses and identify the specific channels through which natural resources affect the fiscal system. Section 5 concludes.

3.2 Resource curse, fiscal capacity and political institutions

There seems to be a consensus in the literature that natural resource abundance may be harmful to tax systems, as governments tend to substitute tax revenues with resource revenues. Part of the literature has discussed this effect with respect to the short-term macroeconomic consequences for taxation, in terms of the amount and composition of tax revenues, as well as spending. According to James (2015), a benevolent government decreases non-resource tax rates and increases spending and savings in response to higher resource revenues. This idea is supported by US-state level data: a $1 increase in resource revenue results in a $0.25 decrease in non-resource revenue, a $0.43 increase in government spending and a $0.32 increase in public savings. Morrison (2009) finds that an increase in non-tax revenue is associated with reduced taxation on elites in democracies and more social spending in dictatorships. Focussing on the consequences for tax composition in resource-rich economies, Crivelli and Gupta (2014) estimate whether there is a differential effect of resource revenue on different components of non-resource taxation, and find a large negative impact of resource revenues on the taxation of goods and services, and a more modest impact on corporate income tax and trade taxes. Looking at tax performance, Morrissey et al. (2016) find that a reliance on natural resources amplifies the negative effects of macroeconomic shocks (terms of trade, exchange rates and natural disasters) on total revenues. Interestingly, they also find that democracies tend to outperform non-democracies in revenue resilience to shocks in lower income countries.

Less attention has been paid to the long-term consequences, i.e. the effect of natural resources on tax system building. In this case, unlike the benevolent government framework, the negative effect of resource rents on taxation can be explained considering the incentives for investing in the tax system. Tax revenues can be used by the government to provide public goods and services and thereby

increase political support. In this contest, windfall revenues shift the marginal benefits from tax revenues, leading to a lower level of tax revenues and a lower-quality tax system (Knack, 2009). The capacity to raise taxes can also be modelled as forward looking investments under uncertainty (Besley and Persson, 2010, 2011). According to the recent literature on state capacity, the government can choose the level of redistributive transfers and public goods provision, but the level of taxation required to implement these policies is constrained by fiscal infrastructures. Incumbents have the opportunity to invest in increased fiscal capacity, but their incentives for building an efficient fiscal apparatus depends on political and economic factors such as the level of income, war, political stability and conflict within the country. Jensen (2010) extends this framework to considering how natural resources affect investments in fiscal capacity, arguing that when a shock renders the economy more resource dependent, investment is discouraged due to the smaller tax base. The ensuing cross-section evidence is consistent with this hypothesis: a 1% increase in resource intensity causes a 1.4% decrease in fiscal capacity.

Although there is agreement on the negative effect of natural resources on fiscal capacity, the actual empirical evidence is fairly limited, often fraught with methodological challenges, and in need of systematic investigation. Moreover, existing studies do not consider a crucial aspect at the heart of our analysis: the interplay between natural resources and institutions. An increasing number of papers argues, and empirically demonstrates, that institutions can mitigate or even reverse the resource curse (e.g., Melhum et al., 2006; Brunnschweiler, 2008; Boschini et al., 2007; El Anshasy and Katsaiti, 2013; Bhattacharyya and Hodler, 2010, 2014; Ebeke et al., 2015; Omgba, 2015).[3] Two explanations have been put forward to understand the role of institutions: the "rent-seeking model" (Tornell and Lane, 1999; Torvik, 2002; Melhum et al., 2006) and the "patronage model"

[3] Andersen and Aslaksen (2008) argue that what matters in reducing negative effects on growth is the constitutional arrangement: presidential regimes and proportional electoral systems are more likely to be afflicted by the resource curse. The detrimental effect of natural resources may also be reversed by high human capital endowments (Kurtz and Brooks, 2011), while public spending could mitigate civil conflicts related to oil wealth (Bodea et al. 2016).

(Robinson et al, 2006; Caselli and Cunningham, 2009).[4] According to the former, the economic institutions governing the private sector are what matter. Resource rents change the preferences of private individuals so they switch from productive to unproductive activities. Thus, natural resources hinder economic growth only if the quality of institutions that govern the profitability of productive enterprise is such that incentivises rent seeking. For example, Melhum et al., 2006 argue that the combination of resource abundance and "grabber friendly" institutions is detrimental for economic development, while "producer friendly" institutions help countries take full advantage of their natural resource endowments. On the contrary, the "patronage model" focuses on the institutions governing the use of public sector resources. Resource booms increase the value of incumbency and provide politicians with more funds which can be used to influence the outcome of elections, thereby increasing resource misallocation in the rest of the economy. However, institutions that promote accountability and state competence discourage the perverse political incentives that such a boom creates.

Perverse effects from rent seeking and patronage are not mutually exclusive but can operate together. But is there an institutional environment where an economy can have both private sector and state institutions that avert rent-seeking and patronage mechanisms? This is where political institutions that place effective constraints on a ruler can play a major role. Such political systems promote contracting and property rights institutions, fostering production activities, so that a large cross-section of society can take advantage of economic opportunities (Acemoglu, Johnson and Robinson 2005). At the same time, limits on executive power promote a "common interest" environment, in which the ruling minority is unable to hand out favours to cronies or themselves (Besley and Persson 2011). In this paper, focussing on these kinds of political institution, we assess if natural resources affect fiscal capacity and if a higher level of checks and balances on executive power can mitigate this effect. Subject to checks and balances, a ruler in a resource-rich economy may be more likely to promote an effective independent

[4] Caselli and Cunningham (2009) define the underlying mechanisms of these models as decentralised and centralised, respectively. Other mechanisms (soft budget constraint and wealth effect) are considered of secondary importance.

civil service (rather than one based on patronage) and to follow the rule of law, so that the judicial system may counter rent seeking more effectively. Figure 1 suggests that a fiscal resource curse does exist: countries with a high level of total natural resource rents collect a low level of taxes as a percentage of GDP. However, splitting the sample into high- and low-level executive constraints (right-hand scatter plot), the effect of resource rents on taxation can be significantly different.[5]

[Figure 1 about here]

The role of natural resource abundance and the mitigating effect that political institutions play in developing fiscal capacity can be restated via two testable hypotheses:

i. *Resource rents reduce the incentives to invest in fiscal capacity, so resource-rich countries have less developed tax systems (collect a lower share of income tax in total taxes).*
ii. *Political institutions placing limits on the executive powers promote common interests and raise the incentives for investing in fiscal capacity. The negative effect of natural resources on fiscal infrastructure is therefore mitigated or neutralised in countries with a higher level of executive constraints.*

The following sections look at how natural resources affect the incentives for building an efficient fiscal apparatus, using different measures of fiscal capacity.

[5] Taxes are defined as the non-resource component of total tax revenues excluding social contributions. Data are averaged from 2000 to 2011, while total natural resource rents are averaged between 1970 and 1999. The sample is split, considering the median value of executive constraints. Variables and sources are described in Table A1 in the Appendix. The apparently heterogeneous effect of natural resource rents is confirmed even when a possible outlier such as Lesotho is excluded from the sample (Figure A1 in the Appendix).

3.3 Empirical strategy and data

The previous section suggests that there may be a non-linear relationship between resource income and fiscal capacity, depending on the type of political institutions. In principle, there are two possible approaches to estimate this relationship.

The first estimates the relationship under investigation using cross-country data in levels, since the types of mechanism we seek to document look at the structural conditions under which countries develop capable states, and are, therefore, long-term in nature. In this case, regressions based on cross-section averages, as shown in Figures 1 and 2, are suitable. However, there are at least two problems with this approach. The first is the vulnerability to omitted variable bias, as there may be several hard-to-capture factors correlated with both the volume of resource rents and state capacity. The second is that shaping the structure of the economy, including its degree of reliance on natural resources, is a process driven by a variety of social forces, including state institutions. Hence, the estimated effect of natural resource reliance could be affected by reverse causality and be subject to bias.

The second approach relies on assessing if the type of relationship documented in figures 1 and 2 disappears when looking at the effect of changes in resource income on fiscal capacity. If it does not, we are probably capturing a causal effect. This approach involves the use of panel methods, conditional at the initial level on political institutions. In particular, looking at the effect of changes in resource income on fiscal capacity eliminates confounding time-invariant country-specific factors. That is, fixed effects can be added to take care of country-specific factors affecting both resource income and fiscal capacity, while time effects can be added to control for global trends.

We use the panel approach, coupled with the choice of a resource income variable allowing clean identification of its effect. We also use resource rent data as a share of national income provided by the World Bank.[6] Such variables are

[6] See World Development Indicators, available at: http://databank.worldbank.org. Resource rent estimation is based on sources and methods fully described by the World Bank (2011) i.e. on the difference between the price of a commodity and the average cost of producing it, estimating the world price of units of specific commodities and subtracting estimates of average unit costs of

based on commodity prices. Assuming that both the identity of a country's commodities and world prices are largely exogenous to state institutions, this measure avoids identification problems related to the estimation of the effects of natural resources (this approach was first used by Caselli and Tesei, 2016). This assumption can be tested, albeit indirectly. We investigate whether it holds by excluding from the sample large commodity producing countries able to influence world prices.

Our specification is the following:

$$FC_t = b_0 + b_1 RR_{it\text{-}4\text{-}bar} + b_2 EC_{it\text{-}4} + b_3 RR_{it\text{-}4\text{-}bar} * EC_{it\text{-}4} + \boldsymbol{b}\mathbf{X}_{\mathbf{it}} + \mu_i + \lambda_t + u_{it}$$

FC_t is fiscal capacity at time *t*. Our measure of fiscal capacity is given by the ratio between non-resource taxes on income, profits, and capital gains and total non-resource tax revenues. Contrary to previous proxies of fiscal capacity, often based on the amount of total taxes as a percentage of GDP, ours distinguishes the capacity to raise taxes from the government's policy choices. Indeed, collecting income taxes requires major investments in fiscal infrastructures compared to other types of taxes (Besley and Persson, 2011: 41-42). Data was taken from the Government Revenues Dataset (GRD) developed at the International Centre for Tax and Development (Prichard et al., 2014). This dataset combines data from several international databases, with marked improvements in data coverage. Crucially, it also allows tax revenue to be distinguished from natural resource and non-resource sources, improving the accuracy of measurement.[7]

$RR_{t\text{-}4\text{-}bar}$ is the resource rent, as described above, averaged over *t*-4 to *t*-1 (with a non-overlapping structure), allowing for possible lags in the reaction of fiscal

extraction or harvesting costs (including a normal return on capital). The unit rents are then multiplied by the quantities countries extract or harvest to determine the rents for each commodity as a share of gross domestic product (GDP). Such measures are based on estimates and therefore are subject to measurement error. However, as long as the noise approximates classic errors in variables case, this is a source of attenuation bias. Therefore, it stacks the odds against our results implying that estimates of the effects of natural resource rents may be conservative.

[7] We use the merged version of the GRD dataset in order not to underestimate the fiscal capacity of countries with a federal system.

authorities to events in the natural resources sector and in the political system.[8] EC_{t-4} captures the quality of political institutions at *t*-4, the beginning of each episode. In line with our hypothesis, it is measured by the executive constraints score (*xconst*) provided by the Polity IV dataset (Marshall et al., 2014). $RR_{it-4-bar}$* EC_{it-4} is the interaction between natural resources and institutional quality.

X_{it} is a set of time-varying controls (also averaged over *t*-4 to *t*-1, with a non-overlapping structure). Some of them are standard variables from the literature on the origins of state capacity, including population density, external war and civil war. Population density should be positively correlated with state capacity, assuming that it is less challenging to develop taxation infrastructures in states where the population is concentrated in urban areas (Herbst, 2000). We use the number of people per square kilometres of land as calculated by the World Bank (2016). External conflicts increase the demand for public services such as defence and thereby increase the incentive to invest in state capacity. On the contrary, civil wars, promoting redistributive interests, hinder the construction of an efficient fiscal apparatus (Besley and Persson, 2010). To capture these effects, we use the hostility level of interstate disputes (Palmer et al., 2015) and the intensity level of internal and internationalised internal armed conflict (UCDP/PRIO, 2016), respectively. Finally, given the nature of our proxy for fiscal capacity, we also add controls that are macroeconomic in nature, as suggested in empirical studies on tax effort (e.g., Crivelli and Gupta 2014): the level of external debt, the sum of exports and imports of goods and services measured as a share of gross domestic product, and the level of aid measured as official development assistance per capita. All the variables are fully described in Appendix A1.

All regressions include country and year dummies (μ_i and λ_t, respectively). Standard errors are clustered at the country level to allow for unknown forms of heteroskedasticity and serial correlation. We study a sample of 100 developing

[8] This approach appears to be standard in resource curse literature (and is in line with other literature, e.g., Caselli and Tesei, 2016, and Bhattacharyya and Hodler, 2010), as well as broader political economy literature investigating institutional factors (e.g., Clomp and de Haan, 2016). Presumably, empirical analyses using a panel with "high frequency" data (e.g., yearly) would fail to properly capture structural effects, such as resource abundance on fiscal capacity.

countries from 1981 to 2011. The descriptive statistics presented in Table 1 show that our key variables vary both across countries and over time. Breaking the period down into decades shows that such a pattern of variation is not driven by any particular sub-period (Table 1, panel b). Table 1 in the Appendix describes variables and sources.

[Table 1 about here]

3.4 Results

This section presents the results, in four steps. We first consider the total amount of resource rents. Then, we decompose it to look at how different natural resources affect fiscal capacity. A series of robustness checks follows. Finally, we unpack the concept of fiscal capacity to identify which fiscal institutions are affected.

3.4.1 The effect of natural resources on fiscal capacity

Table 2 presents our baseline results. Columns 1-3, where the variable of interest enters in linear form, show a negative but not significant effect of total natural resource rents on fiscal capacity. However, this result could have been driven by the possibly non-linear relationship between resource rents and fiscal capacity. Indeed, the coefficient of total natural resource rents is significant once an interaction term is inserted between resource rents and our measure of institutional quality. Column 4 shows that on average fiscal capacity tends to be lower when countries experience an increase in resource rents. However, the interaction term appears significantly positive, suggesting that the negative effect of resource rents diminishes when the level of executive constraints increases. This result holds when the set of controls is included (Column 5). External debt, population density and civil war are significant and have the expected signs: fiscal capacity is higher for less sparsely populated states, whereas external debt and civil war decrease the investments in tax infrastructures.

[Table 2 about here]

Column 1 in table 4 shows the marginal effects of total natural resource rents at different levels of institutional quality. The results confirm our hypotheses: resource rents negatively affect fiscal capacity when the level of executive constraints is very low, but they can even be a blessing for countries where the executive is effectively limited by accountability groups. For countries, such as Uzbekistan, where constitutional restrictions on executive action are ignored (xconst=1), a 1% increase in total natural resource rents would reduce the ability to raise direct taxes, our proxy for fiscal capacity, by approximately 0.41 percentage points. On the other hand, in countries with the highest level of executive constraints (e.g. Albania and Costa Rica), the same increase in resource rents would improve fiscal capacity by 0.37 percent. [9] Considering that the (within) standard deviation in resource rents is above three percentage points, such effects also appear to be economically significant.

Next, we assess the effect of specific natural resources. Some studies suggest that the resource curse may by driven by specific types of natural resource endowments (Isham et al., 2005; Boschini et al., 2007), hence we consider individual components of total natural resource rents: forest, oil, gas, and mineral rents.[10] Disaggregating the effects, Table 3 indicates that none of the four types of resource rents is the sole force driving the heterogeneous effect on fiscal capacity. The general message remains that natural resources may be a curse or a blessing,

[9] For the sake of thoroughness, we have also considered the partial effect of executive constraints at different levels of resource rents. For instance, natural resource rents in developing economies can stifle democratic governance and political institutions via rent-seeking activities by influential private actors or through patronage by the local elite. Hence, an alternative interpretation to our interaction term is that developing economies with less natural resource income may be less prone to such effects. To test this hypothesis, we estimated the magnitude and significance of the partial effect of executive constraints calculated at different levels of resource rents. Such estimates, available on request, show that the effect of executive constraints can decrease in magnitude and even change sign in environments with higher resource rents. However, there is no evidence that such effects are significant at conventional levels.

[10] The World Bank also provides data for coal rents, but they are ignored due to the lack of variation (very few observations are different from zero).

depending on the level of executive constraints. The political institutions-rents interaction term is positive and significant in all estimations except for mineral rents (where it is marginally significant). However, these results also suggest that different resource rents affect fiscal capacity differently.

[Tables 3 about here]

In the last column of table 3, we include all resource rent variables in the regression. Table 4 shows the marginal effects calculated using the coefficients from this regression. Interestingly, the negative effect on fiscal capacity is mainly due to forest and mineral rents, but vanishes when the level of executive constraints is at least 4. This echoes earlier findings on minerals and health outcomes (Edwards 2016), but crucially extends and qualifies them, suggesting that negative effects may not materialise, depending on the nature of political institutions; whereas oil and gas rents either have no effect or can actually foster investments in fiscal infrastructures if the level of executive constraints is high (xconst≥3). This partly contradicts initial empirical findings on the negative effects of point-source resources, while offering some support to those who cast doubt on the apparent curse of oil resources for growth and governance (Aleexev and Conrad 2009).

[Table 4 about here]

3.4.2 Robustness checks

The above findings are robust to controlling for all time-invariant variables and for a number of time-varying variables included in the regressions, as well as common trends. However, these results are based on the assumption that resource rents, measured on the basis of international commodity prices, are exogenous to a country's institutions, whereas our results may be driven by large commodity producers, who can influence world commodity prices, raising endogeneity concerns with respect to our variable. Therefore all OPEC members and countries accounting for more than 3% of total world production of a certain commodity have

been excluded from the sample.[11] In all our key regressions, the result on the heterogonous impact of natural resources proves to be robust (Table 5, columns 5-8).

Finally, we present further robustness checks based on the importance of natural resource rents for the economy. We exclude countries in the top and bottom decile of resource rents. Excluding the bottom and top decile, the baseline results are confirmed.

[Tables 5-9 about here]

3.4.3 How do resource rents affect fiscal capacity?

Our findings indicate that political institutions limiting executive power tend to alleviate (or even reverse) the negative effects that reliance on natural resources can have on fiscal systems. However, we have not identified the specific fiscal institutions affected, an exercise that could deliver insights into the specific channels of causation. However, we do consider two possible channels. Unpacking the concept of fiscal capacity, we distinguish between two aspects of fiscal institutions: the accountability and transparency of fiscal institutions, *impartiality*, and their *effectiveness* in extracting revenues.

Impartiality concerns fairness in the exercise of taxation powers: it is the ability of tax systems to make the state accountable to, and transparent for, its citizens, building state-society relations leading to quasi-compliance (e.g., Levi 1988). The other concerns their effectiveness in raising tax revenues, i.e., the ability to coerce citizens to pay taxes. Outcome-based measures of fiscal capacity, such as the tax to GDP ratio or the measure used so far, cannot differentiate between these two quite different dimensions of fiscal systems related to the exercise of taxation powers.

To test whether a fiscal resource curse works through *impartiality* or *effectiveness* (or both), we use a recently created set of indicators provided by PEFA

[11] We identify big producers following the example of Caselli and Tesei (2016).

(2006), the Public Expenditure and Financial Accountability project developed by a partnership of national and international organizations (e.g., IMF and the World Bank).[12] In particular, we use six indicators selected from the PEFA database, neatly capturing the *impartiality* and *effectiveness* of tax systems. They are described below:[13]

1. *Transparency of taxpayer obligations and liabilities*, which evaluates taxpayers' access to information on tax liabilities and administrative procedures;
2. *Tax appeals*: assessing the functioning of a tax appeals mechanism;
3. *Controls in the taxpayer registration system*, assessing the quality and maintenance of a taxpayer database;
4. *Effectiveness of penalties for non-compliance*: this addresses failures in registration and tax declaration obligations assessing whether penalties for all areas of non-compliance are set sufficiently high to act as deterrence and are consistently administered;
5. *Quality of tax audit* evaluates whether and how tax audits and fraud investigations are undertaken;
6. *Effectiveness of transfer of tax collections to the Treasury by the revenue administration* assesses how often revenue collections are transferred to the Treasury.

The first two indicators capture the *impartiality* of fiscal capacity, since they hinge on the relationship between the State and the public: empowering it against the taxation power of the former or making such power clearly defined and not subject to discretion. The final four measures assess the coercive aspects of the tax system: they are all desirable features of a tax machine aiming at raising revenues.[14]

[12] See www.pefa.org for a presentation of the project, its aims and the data.

[13] Appendix 1 sets out detailed definitions and scales of assessment for our six PEFA indicators. Full details of the PEFA framework, indicators and assessment method are given in the database codebook at http://www.pefa.org/sites/pefa.org/files/attachments/PMFEng-finalSZreprint04-12_1.pdf.

[14] Methodologically, these are *de facto* measures: what matters is the actual working of the system and not what is merely written in the law. This ensures that the assessment is based on institutional

Higher scores indicate greater levels of fiscal capacity: both *impartiality* and *effectiveness*. The table below presents OLS cross-section regressions for over sixty developing economies, where each of the above fiscal institution measures acts as a dependent variable.[15]

[Table 10 about here]

Subject to the limitations of cross-section estimates discussed earlier, the above results suggest that the effect of natural resources is likely to work through fiscal institutions relating to *impartiality*, while the evidence that they affect *effectiveness* is weak. The related marginal effects, in particular, indicate that a fiscal resource curse exists only in political systems with low levels of checks and balances on executive power. The curse disappears, or becomes a blessing, in economies that can successfully limit the power of the executive. Under such political conditions, the fiscal bargain between a ruler and citizens, at the heart of the construction of a fiscal state (Brautigam et al 2008), may be facilitated. Therefore, it seems that developing both fiscal capacity and the natural resources sector, is possible, without any tradeoff.

[Table 11 about here]

reforms, reacting to the pressure of external authorities, to some degree internalised by those who implement them.

[15] Although the PEFA dataset is gradually expanding, its structure is such that it does not yet allow for panel analysis. In particular, PEFA variables range only from 2005 to 2013 and have a T-bar of 1.5, as well as exhibiting very little variation within countries.

3.5 Conclusions

This paper investigates how natural resources affect the incentives for investing in fiscal capacity, and the role of political institutions in this process. Based on previous studies demonstrating that institutions can mitigate or even reverse the resource curse, we posit that the negative effect of resource rents on the ability of states to raise revenues can vanish if political institutions effectively limit executive power. Using panel data covering the period 1981-2011 for 100 developing countries, we find that resource rents are negatively associated with fiscal capacity, measured as the share of non-resource taxes on income, profits, and capital gains in non-resource total taxes. However, countries with a high level of executive constraints are able to neutralise or even reverse this effect, depending on the type of resource endowments. Further analysis shows that the effect of natural resources is likely to work through fiscal institutions that make the state accountable and transparent to its citizens. Our findings indicate that, in polities providing strong checks and balances on the executive power, it is possible to develop both fiscal capacity and the natural resources sector. Whether a fiscal resource curse exists or not is a question of what type of political institutions countries have adopted.

References

Acemoglu, D., Johnson, S., Robinson, J. A., Yared, P. (2005). 'From Education to Democracy?'. *American Economic Review*, 95(2): 44-49.

Alexeev, M., Conrad, R. (2009). 'The elusive curse of oil'. *The Review of Economics and Statistics*, 91(3): 586-598.

Besley, T., Persson, T. (2010). 'State capacity, conflict, and development'. *Econometrica*, 78(1): 1-34.

Besley, T., Persson, T. (2011). *Pillars of prosperity: The political economics of development clusters*. Princeton University Press.

Bhattacharyya, S., Hodler, R. (2010). 'Natural resources, democracy and corruption'. *European Economic Review*, 54(4): 608-621.

Bhattacharyya, S., Hodler, R. (2014). 'Do natural resource revenues hinder financial development? The role of political institutions'. *World Development*, 57: 101-113.

Bodea, C., Higashijima, M., & Singh, R. J. (2016). 'Oil and Civil Conflict: Can Public Spending Have a Mitigation Effect?'. *World Development*, 78: 1-12.

Boschini, A. D., Pettersson, J., Roine, J. (2007). 'Resource curse or not: A question of appropriability'. *The Scandinavian Journal of Economics*, 109(3): 593-617.

Brautigam, D., Fjeldstad, O.H. and Moore, M. (2008). *Taxation and state-building in developing countries: capacity and consent,* Cambridge: Cambridge University Press.

Brunnschweiler, C. N. (2008). 'Cursing the blessings? Natural resource abundance, institutions, and economic growth'. *World development*, 36(3): 399-419.

Carmignani, F. 2013, 'Development outcomes, resource abundance, and the transmission through inequality', *Resource and Energy Economics,* vol. 35, pp. 412-428

Caselli, F., Cunningham, T. (2009). 'Leader behaviour and the natural resource curse'. *Oxford Economic Papers*, 61(4): 628-650.

Caselli, F., Michaels, G. (2013). 'Do oil windfalls improve living standards? Evidence from Brazil'. *American Economic Journal: Applied Economics*, 5(1): 208-238.

Caselli, F., Tesei, A. (2016). 'Resource windfalls, political regimes, and political stability'. Review of Economics and Statistics, 98(3): 573-590.

Cheibub, J. A. (1998). 'Political regimes and the extractive capacity of governments: Taxation in democracies and dictatorships'. *World Politics*, 50(03): 349-376.

Cotet, A. M., Tsui, K. K. (2013). 'Oil and conflict: What does the cross country evidence really show?'. *American Economic Journal: Macroeconomics*, 5(1): 49-80.

Crivelli, E., Gupta, S. (2014). 'Resource blessing, revenue curse? Domestic revenue effort in resource-rich countries'. *European Journal of Political Economy*, 35: 88-101.

Ebeke, C., Omgba, L. D., & Laajaj, R. (2015). 'Oil, governance and the (mis) allocation of talent in developing countries'. *Journal of Development Economics*, 114: 126-141.

Edwards, Ryan B.(2016) Mining away the Preston curve, World Development, Volume 78, February, Pages 22-36

Ehrhart, H. (2012). 'Assessing the relationship between democracy and domestic taxes in developing countries'. *Economics Bulletin*, 32(1): 551-566.

El Anshasy, A. A., Katsaiti, M. S. (2013). 'Natural resources and fiscal performance: Does good governance matter?'. *Journal of Macroeconomics*, 37: 285-298.

Fum R.M., R. Hodler (2010) 'Natural resources and income inequality: The role of ethnic divisions', *Economics Letters*, 107(3): 360-363.

Gylfason, T. (2001). 'Natural Resources, education, and economic development'. *European Economic Review*, 45: 847-859.

Goderis, Benedikt and Malone, Samuel W. (2011) 'Natural Resource Booms and Inequality: Theory and Evidence'. *Scandinavian Journal of Economics*, 113(2): 388-417.

Haber, S., Menaldo, V. (2011). 'Do natural resources fuel authoritarianism? A reappraisal of the resource curse'. *American political science Review*, 105(01): 1-26.

Herbst, J. (2000). *States and Power in Africa: Comparative Lessons in Authority and Control*. Princeton, NJ: Princeton University Press.

Isham, J., Woolcock, M., Pritchett, L., Busby, G. (2005). 'The varieties of resource experience: natural resource export structures and the political economy of economic growth'. The World Bank Economic Review, 19(2): 141-174.

James, A. (2015). 'US State fiscal policy and natural resources'. *American Economic Journal: Economic Policy*, 7(3): 238-257.

Jensen, A.D. (2011). 'State-Building in Resource-Rich Economies'. *Atlantic Economic Journal*, 39: 171-193.

Levi, M. (1988). *Of Rule and Revenue*, Berkeley: University of California Press.

Knack, S. (2009). 'Sovereign rents and quality of tax policy and administration'. *Journal of Comparative Economics*, 37(3): 359-371.

Klomp, J., de Haan, J. (2016). 'Election cycles in natural resource rents: Empirical evidence'. *Journal of Development Economics*, 121: 79-93.

Kurtz, M. J., Brooks, S. M. (2011). Conditioning the "resource curse": globalization, human capital, and growth in oil-rich nations. Comparative Political Studies, 44(6): 747-770.

Marshall, M.G., T.R. Gurr, K. Jaggers (2014). 'Polity IV Project: Political Regime Characteristics and Transitions, 1800–2013'. Available at: http://www.systemicpeace.org/polity/polity4.htm

Mehlum, H., Moene, K., Torvik, R. (2006). 'Institutions and the resource curse'. *The economic journal*, 116(508): 1-20.

Morrison, K. M. (2009). 'Oil, nontax revenue, and the redistributional foundations of regime stability'. *International Organization*, 63(01), 107-138.

Morrissey, O., Von Haldenwang, C., Von Schiller, A., Ivanyna, M., Bordon, I. (2016). 'Tax revenue performance and vulnerability in developing countries'. *The Journal of Development Studies*, 1-15.

Omgba, L. D. (2015). 'Why do some oil-producing countries succeed in democracy while others fail?'. *World Development*, 76: 180-189.

Palmer, G., V. D'Orazio, M. Kenwick, and M. Lane (2015). 'The MID4 Data Set: Procedures, Coding Rules, and Description.' Conflict Management and Peace Science, 32(2): 222-24.

Pineda José and Francisco Rodríguez (2010), "Curse or Blessing? Natural Resources and Human Development", *Human Development Research Paper 2010/04*

Prichard, W., Cobham, A., Goodall, A. (2014) "The ICTD Government Revenue Dataset", ICTD Working Paper 34.

Rodriguez, F., Sachs, J. D. (1999). 'Why do resource-abundant economies grow more slowly?'. *Journal of Economic Growth*, 4(3): 277-303.

Ross, M. L. (2001). 'Does oil hinder democracy?'. *World politics*, 53(3): 325-361.

Sachs, J.D., Warner, A.M. (1995). 'Natural resource abundance and economic growth'. *NBER Working Paper*, w5398.

Sachs, J. D., Warner, A. M. (1999). 'The big push, natural resource booms and growth'. *Journal of development economics*, 59(1): 43-76.

Stijns, J. P. (2006). 'Natural resource abundance and human capital accumulation'. *World Development*, 34(6):1060-1083.

Tornell, A., Lane, P. R. (1999). 'The voracity effect'. *American economic review*, 89(1): 22-46.

Torvik, R. (2002). 'Natural resources, rent seeking and welfare'. *Journal of development economics*, 67(2): 455-470.

UCDP/PRIO (2016). *Armed Conflict Dataset*. Available at *https://www.prio.org/Data/Armed-Conflict/UCDP-PRIO/*.

Van der Ploeg, F. (2011). 'Natural Resources: Curse or Blessing?'. *Journal of Economic Literature*, 49(2): 366-420.

World Bank (2011). *The Changing Wealth of Nations: Measuring Sustainable Development in the New Millennium*, Washington, DC. Available at *http://siteresources.worldbank.org/ENVIRONMENT/Resources/ChangingWealthNations.pdf*.

World Bank (2016). *World Development Indicators*, Washington, DC. Available at *http://data.worldbank.org/data-catalog/world-development-indicators*.

Table 3.1 – Summary Statistics

Panel (a)	Observations	Mean	Std. Dev			Minimum	Maximum
			overall	between	within		
Fiscal capacity	350	29.858	11.827	10.286	5.947	7.052	68.692
Executive constraints	350	4.446	2.003	1.711	1.200	1	7
Total natural resources rents	350	8.497	10.954	12.878	3.212	0.003	70.624
Forest rents	350	3.449	5.685	5.893	2.196	0	41.770
Oil rents	350	3.028	8.242	10.372	1.597	0	50.107
Gas rents	350	0.895	4.105	5.192	1.275	0	55.528
Mineral rents	350	1.045	2.863	2.718	1.432	0	20.618
Political stability	350	3.093	1.187	1.007	0.485	2	7
External Debt	350	71.510	87.168	78.008	53.229	2.725	759.970
Trade	350	75.696	38.127	35.851	14.345	13.037	253.047
Net ODA and aid per capita	350	71.208	84.082	76.157	34.113	-3.785	620.926
Population density	350	98.265	142.726	139.294	18.085	1.462	1145.363
External war	350	0.955	1.326	1.252	0.704	0	5
Civil war	350	0.238	0.492	0.335	0.340	0	2

Panel (b)	1981 - 1990 Std. Dev			1991 - 2000 Std. Dev			2001 - 2011 Std. Dev		
	overall	between	within	overall	between	within	overall	between	within
Fiscal capacity	12.348	11.502	3.654	12.106	11.342	4.415	11.642	10.982	4.077
Executive constraints	2.050	1.960	0.762	1.992	1.776	1.017	1.829	1.736	0.672
Total natural resources rents	12.983	11.753	3.487	7.742	9.040	1.758	12.920	14.113	3.172
Forest rents	8.665	7.330	2.480	5.251	4.837	1.170	5.287	6.003	1.201
Oil rents	11.167	9.922	2.398	5.631	7.999	1.078	8.418	10.644	1.161
Gas rents	0.648	0.731	0.095	1.212	1.351	0.278	6.553	6.266	1.583
Mineral rents	2.962	4.608	0.358	2.625	2.306	0.788	2.982	2.663	1.483

Table 3.2 - Baseline results for fiscal capacity and total natural resources rents

VARIABLES	(1) Fiscal capacity	(2) Fiscal capacity	(3) Fiscal capacity	(4) Fiscal capacity	(5) Fiscal capacity
Total natural resources rents	-0.127	-0.122	-0.0759	-0.591***	-0.544**
	(0.157)	(0.158)	(0.194)	(0.223)	(0.254)
Executive constraints		0.238	0.360	-0.612	-0.522
		(0.354)	(0.458)	(0.432)	(0.493)
Total natural resources rents*Executive constraints				0.126***	0.131***
				(0.0315)	(0.0368)
Political Stability			0.651		0.461
			(0.950)		(0.870)
External Debt			-0.0188**		-0.0165***
			(0.00747)		(0.00578)
Trade			0.0211		0.00306
			(0.0302)		(0.0296)
Net ODA and aid per capita			-0.00353		-0.0114
			(0.0139)		(0.0138)
Population density			0.0521*		0.0588**
			(0.0269)		(0.0265)
External war			0.502		0.482
			(0.720)		(0.679)
Civil war			-2.638**		-2.947**
			(1.317)		(1.303)
Constant	31.50***	30.65***	22.99***	35.43***	29.91***
	(2.480)	(2.620)	(5.530)	(3.219)	(5.794)
Observations	350	350	350	350	350
Number of country_id	91	91	91	91	91
Adjusted R-squared	0.094	0.093	0.148	0.144	0.200
Year FE	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
Joint(p)				0.000615	0.00217

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3 - Fiscal capacity and different natural resources rents

VARIABLES	(1) Forest Rents	(2) Forest Rents	(3) Oil Rents	(4) Oil Rents	(5) Gas Rents	(6) Gas Rents	(7) Mineral Rents	(8) Mineral Rents	(9) Rents
Executive constraints	0.389	-0.229	0.295	0.0407	0.301	0.254	0.290	0.226	-0.602
	(0.447)	(0.501)	(0.447)	(0.475)	(0.444)	(0.440)	(0.441)	(0.421)	(0.509)
Forest rents	-0.347	-0.918***							-1.108***
	(0.243)	(0.179)							(0.177)
Forest rents*Executive constraints		0.163***							0.169***
		(0.0519)							(0.0548)
Oil rents			0.165	-0.123					-0.289
			(0.324)	(0.359)					(0.364)
Oil rents*Executive constraints				0.122***					0.122***
				(0.0429)					(0.0386)
Gas rents					0.172	-0.238			0.410
					(0.281)	(0.183)			(0.464)
Gas rents*Executive constraints						0.175**			0.0354
						(0.0739)			(0.0864)
Mineral rents							-0.407**	-0.881	-1.244**
							(0.192)	(0.544)	(0.588)
Mineral rents*Executive constraints								0.117	0.180
								(0.124)	(0.126)
Political Stability	0.639	0.477	0.134	-0.0799	0.233	0.180	0.235	0.304	0.273
	(0.877)	(0.853)	(1.009)	(0.977)	(0.879)	(0.859)	(0.882)	(0.907)	(0.916)
External Debt	-0.0148**	-0.0124**	-0.0210***	-0.0209***	-0.0206***	-0.0204***	-0.0224***	-0.0229***	-0.0133***
	(0.00721)	(0.00554)	(0.00681)	(0.00653)	(0.00656)	(0.00631)	(0.00756)	(0.00799)	(0.00487)
Trade	0.0248	0.00273	0.0197	0.0225	0.0212	0.0172	0.0239	0.0204	-7.97e-05
	(0.0293)	(0.0316)	(0.0283)	(0.0279)	(0.0279)	(0.0280)	(0.0271)	(0.0284)	(0.0316)
Net ODA and aid per capita	0.000591	-0.0124	-0.00453	-0.00288	-0.00487	-0.00331	-0.00746	-0.00753	-0.0113
	(0.0147)	(0.0156)	(0.0137)	(0.0134)	(0.0137)	(0.0138)	(0.0140)	(0.0140)	(0.0146)
Population density	0.0516**	0.0530**	0.0590**	0.0622**	0.0587**	0.0616**	0.0558**	0.0563**	0.0569**
	(0.0255)	(0.0249)	(0.0264)	(0.0263)	(0.0261)	(0.0262)	(0.0252)	(0.0256)	(0.0250)
External war	0.523	0.424	0.464	0.495	0.435	0.408	0.399	0.426	0.299
	(0.723)	(0.714)	(0.729)	(0.704)	(0.729)	(0.729)	(0.705)	(0.710)	(0.674)
Civil war	-2.794**	-2.816**	-2.680*	-2.806**	-2.727**	-2.729**	-2.566*	-2.657*	-3.070**
	(1.311)	(1.280)	(1.363)	(1.365)	(1.360)	(1.364)	(1.316)	(1.351)	(1.304)
Constant	22.54***	28.28***	23.55***	25.35***	23.65***	23.92***	24.89***	25.12***	32.24***
	(5.781)	(5.905)	(5.636)	(5.849)	(5.584)	(5.497)	(5.553)	(5.528)	(5.862)
Observations	350	350	355	355	355	355	355	355	350
Number of country_id	91	91	92	92	92	92	92	92	91
Adjusted R-squared	0.160	0.193	0.148	0.166	0.149	0.162	0.154	0.155	0.238
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Joint(p)		8.69e-06		0.0200		0.0647		0.0686	8.55e-09

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.4 – Marginal effects of resource rents at different levels of executive constraints

executive constraints	Total natural resources rents b/se	Forest rents b/se	Oil rents b/se	Gas rents b/se	Mineral rents b/se
1	-0.413* (0.23)	-0.939*** (0.14)	-0.166 (0.36)	0.446 (0.39)	-1.064** (0.47)
2	-0.282 (0.21)	-0.771*** (0.12)	-0.044 (0.37)	0.481 (0.33)	-0.884** (0.37)
3	-0.151 (0.19)	-0.602*** (0.12)	0.079 (0.37)	0.516* (0.28)	-0.705** (0.28)
4	-0.020 (0.18)	-0.434*** (0.14)	0.201 (0.38)	0.552** (0.24)	-0.525** (0.23)
5	0.111 (0.17)	-0.265 (0.18)	0.323 (0.40)	0.587** (0.24)	-0.345 (0.24)
6	0.242 (0.18)	-0.097 (0.23)	0.446 (0.41)	0.623** (0.27)	-0.165 (0.31)
7	0.373** (0.19)	0.072 (0.28)	0.568 (0.43)	0.658** (0.32)	0.015 (0.40)

Notes: The marginal effects of specific natural resources are calculated using the coefficients from Table 3, Column 9.

Table 3.5 - Fiscal capacity and different natural resources rents – Robustness checks

	Excluding bottom decile		Excluding top decile		Excluding big producers		Excluding OPEC countries	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Total Rents	Rents	Total Rents	Rents	Total Rents	Rents	Total Rents	Rents
Executive constraints	-0.219	-0.270	-0.617	-0.443	-0.844*	-0.872*	-0.443	-0.371
	(0.548)	(0.591)	(0.504)	(0.619)	(0.501)	(0.477)	(0.516)	(0.569)
Total natural resources rents	-0.482*		-1.033***		-0.832***		-0.943***	
	(0.252)		(0.346)		(0.199)		(0.220)	
Total natural resources rents*Executive constraints	0.101***		0.173***		0.173***		0.171***	
	(0.0384)		(0.0614)		(0.0408)		(0.0487)	
Forest rents		-0.992***		-1.529***		-1.187***		-1.127***
		(0.179)		(0.402)		(0.167)		(0.186)
Forest rents*Executive constraints		0.124**		0.216***		0.216***		0.173***
		(0.0608)		(0.0771)		(0.0557)		(0.0598)
Oil rents		-0.237		-0.248		-0.429		-0.759
		(0.366)		(0.502)		(0.427)		(0.610)
Oil rents*Executive constraints		0.101**		0.00158		0.104*		0.101
		(0.0425)		(0.101)		(0.0551)		(0.173)
Gas rents		0.337		-0.186		-0.646		0.0328
		(0.377)		(1.323)		(0.451)		(0.365)
Gas rents*Executive constraints		0.0318		0.142		0.563**		0.102
		(0.0707)		(0.191)		(0.267)		(0.0697)
Mineral rents		-1.152*		-1.314*		-1.200**		-1.418**
		(0.605)		(0.702)		(0.555)		(0.580)
Mineral rents*Executive constraints		0.142		0.183		0.216		0.208
		(0.130)		(0.143)		(0.143)		(0.126)
Political Stability	0.613	0.413	0.227	0.680	-0.282	-0.0825	0.782	0.798
	(0.948)	(0.995)	(0.939)	(1.012)	(0.861)	(0.819)	(0.934)	(0.974)
External Debt	-0.0163***	-0.0133**	-0.0181**	-0.0149**	-0.0141***	-0.0110**	-0.0123**	-0.0113**
	(0.00595)	(0.00515)	(0.00694)	(0.00691)	(0.00504)	(0.00481)	(0.00507)	(0.00481)
Trade	0.0262	0.0248	0.0195	0.0203	0.00921	-0.00408	0.00757	0.00758
	(0.0305)	(0.0321)	(0.0371)	(0.0368)	(0.0312)	(0.0317)	(0.0323)	(0.0326)
Net ODA and aid per capita	-0.0118	-0.00957	-0.0178	-0.0114	-0.0199	-0.0209	-0.0159	-0.0152
	(0.0150)	(0.0169)	(0.0128)	(0.0158)	(0.0152)	(0.0165)	(0.0139)	(0.0158)
Population density	0.0498*	0.0470*	0.0793***	0.0722**	0.0737***	0.0667**	0.0721**	0.0673**
	(0.0281)	(0.0268)	(0.0299)	(0.0295)	(0.0276)	(0.0266)	(0.0284)	(0.0280)
External war	1.044*	0.855	0.301	0.103	0.382	0.234	0.267	0.159
	(0.541)	(0.534)	(0.702)	(0.725)	(0.733)	(0.759)	(0.716)	(0.722)
Civil war	-2.229*	-2.382**	-2.927**	-2.675*	-2.108	-1.999	-3.038**	-2.975**
	(1.130)	(1.161)	(1.360)	(1.367)	(1.326)	(1.324)	(1.346)	(1.356)
Constant	25.11***	27.32***	30.33***	28.59***	33.63***	34.31***	29.53***	30.29***
	(5.696)	(5.717)	(6.266)	(6.954)	(5.864)	(5.533)	(6.412)	(6.862)
Observations	313	313	315	315	296	296	326	326
Number of country_id	84	84	79	79	78	78	84	84
Adjusted R-squared	0.232	0.270	0.203	0.218	0.222	0.249	0.217	0.227
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Joint(p)	0.0354	1.38e-08	0.0140	0.000383	5.81e-05	2.22e-10	0.000255	6.07e-07

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.6 – Marginal effects of resource rents at different levels of executive constraints - Excluding bottom decile

executive constraints	Total natural resources rents b/se	Forest rents b/se	Oil rents b/se	Gas rents b/se	Mineral rents b/se
1	-0.381* (0.23)	-0.868*** (0.14)	-0.136 (0.36)	0.369 (0.32)	-1.010** (0.49)
2	-0.280 (0.20)	-0.744*** (0.11)	-0.034 (0.37)	0.401 (0.27)	-0.869** (0.38)
3	-0.179 (0.18)	-0.620*** (0.12)	0.067 (0.38)	0.433* (0.23)	-0.727** (0.29)
4	-0.077 (0.17)	-0.496*** (0.15)	0.168 (0.39)	0.464** (0.21)	-0.585** (0.23)
5	0.024 (0.17)	-0.371* (0.20)	0.269 (0.40)	0.496** (0.21)	-0.444* (0.25)
6	0.125 (0.18)	-0.247 (0.25)	0.371 (0.42)	0.528** (0.24)	-0.302 (0.32)
7	0.226 (0.19)	-0.123 (0.30)	0.472 (0.45)	0.560** (0.28)	-0.161 (0.42)

Notes: The marginal effects of specific natural resources are calculated using the coefficients from Table 5, Columns 1 and 2

Table 3.7 – Marginal effects of resource rents at different levels of executive constraints - Excluding top decile

executive constraints	Total natural resources rents b/se	Forest rents b/se	Oil rents b/se	Gas rents b/se	Mineral rents b/se
1	-0.860*** (0.29)	-1.313*** (0.34)	-0.247 (0.45)	-0.044 (1.14)	-1.131** (0.57)
2	-0.687*** (0.24)	-1.097*** (0.30)	-0.245 (0.42)	0.098 (0.95)	-0.948** (0.45)
3	-0.515*** (0.20)	-0.882*** (0.26)	-0.243 (0.41)	0.240 (0.77)	-0.766** (0.34)
4	-0.342** (0.17)	-0.666*** (0.25)	-0.242 (0.43)	0.381 (0.59)	-0.583** (0.27)
5	-0.169 (0.15)	-0.450* (0.26)	-0.240 (0.46)	0.523 (0.43)	-0.400 (0.27)
6	0.004 (0.16)	-0.234 (0.29)	-0.239 (0.52)	0.665** (0.30)	-0.217 (0.33)
7	0.176 (0.20)	-0.019 (0.33)	-0.237 (0.58)	0.807*** (0.27)	-0.035 (0.44)

Notes: The marginal effects of specific natural resources are calculated using the coefficients from Table 5, Columns 3 and 4

Table 3.8 – Marginal effects of resource rents at different levels of executive constraints - Excluding big producers

executive constraints	Total natural resources rents b/se	Forest rents b/se	Oil rents b/se	Gas rents b/se	Mineral rents b/se
1	-0.659*** (0.18)	-0.971*** (0.13)	-0.325 (0.40)	-0.083 (0.33)	-0.985** (0.43)
2	-0.487*** (0.16)	-0.755*** (0.10)	-0.221 (0.38)	0.480 (0.39)	-0.769** (0.33)
3	-0.314* (0.16)	-0.539*** (0.10)	-0.116 (0.37)	1.043* (0.58)	-0.553** (0.26)
4	-0.141 (0.17)	-0.323** (0.13)	-0.012 (0.37)	1.606** (0.82)	-0.338 (0.26)
5	0.031 (0.18)	-0.107 (0.17)	0.092 (0.37)	2.169** (1.07)	-0.122 (0.34)
6	0.204 (0.20)	0.109 (0.22)	0.196 (0.38)	2.732** (1.33)	0.094 (0.44)
7	0.377 (0.23)	0.325 (0.27)	0.300 (0.40)	3.295** (1.59)	0.310 (0.57)

Notes: The marginal effects of specific natural resources are calculated using the coefficients from Table 5, Columns 5 and 6

Table 3.9 – Marginal effects of resource rents at different levels of executive constraints - Excluding OPEC countries

executive constraints	Total natural resources rents b/se	Forest rents b/se	Oil rents b/se	Gas rents b/se	Mineral rents b/se
1	-0.772*** (0.18)	-0.954*** (0.14)	-0.658 (0.47)	0.135 (0.32)	-1.209*** (0.46)
2	-0.601*** (0.15)	-0.781*** (0.12)	-0.556 (0.36)	0.237 (0.28)	-1.001*** (0.36)
3	-0.430*** (0.13)	-0.607*** (0.12)	-0.455 (0.31)	0.339 (0.25)	-0.793*** (0.26)
4	-0.259** (0.12)	-0.434*** (0.14)	-0.353 (0.36)	0.442* (0.24)	-0.584*** (0.21)
5	-0.088 (0.13)	-0.261 (0.19)	-0.252 (0.46)	0.544** (0.26)	-0.376* (0.23)
6	0.083 (0.16)	-0.088 (0.24)	-0.151 (0.60)	0.646** (0.28)	-0.168 (0.30)
7	0.254 (0.20)	0.086 (0.29)	-0.049 (0.75)	0.748** (0.33)	0.041 (0.40)

Notes: The marginal effects of specific natural resources are calculated using the coefficients from Table 5, Column 11

Table 3.10 - Tests of possible channels of causation from resource rents to fiscal capacity

VARIABLES	(1) Transparency of taxpayer obligations and liabilities	(2) Transparency of taxpayer obligations and liabilities	(3) Tax appeals	(4) Tax appeals	(5) Controls in the taxpayer registration system	(6) Controls in the taxpayer registration system	(7) Effectiveness of penalties for non-compliance with registration and tax declaration	(8) Effectiveness of penalties for non-compliance with registration and tax declaration	(9) Quality of tax audit	(10) Quality of tax audit	(11) Effectiveness of transfer of tax collections to the Treasury by the revenue administration.	(12) Effectiveness of transfer of tax collections to the Treasury by the revenue administration.
Total natural resources rents	-0.00945	-0.0409**	-0.00498	-0.0229	-0.00881	-0.00135	-0.0116	-0.0437*	-0.000753	0.00435	-0.000799	-0.0176
	(0.00982)	(0.0162)	(0.0103)	(0.0212)	(0.00836)	(0.0193)	(0.0160)	(0.0225)	(0.0116)	(0.0170)	(0.0133)	(0.0313)
Executive constraints	0.148	0.0659	0.244***	0.197**	0.295***	0.315***	0.238**	0.154	0.108	0.121	0.0779	0.0342
	(0.0898)	(0.0997)	(0.0822)	(0.0879)	(0.0653)	(0.0778)	(0.0925)	(0.0998)	(0.0827)	(0.0945)	(0.0759)	(0.0902)
Total natural resources rents*Executive constraints		0.0118**		0.00670		-0.00280		0.0121*		-0.00192		0.00630
		(0.00449)		(0.00643)		(0.00574)		(0.00629)		(0.00388)		(0.00756)
polstab8104	0.0367	0.000691	0.108	0.0875	-0.0605	-0.0520	0.0680	0.0308	-0.325**	-0.319**	-0.0426	-0.0619
	(0.144)	(0.145)	(0.111)	(0.112)	(0.133)	(0.134)	(0.153)	(0.161)	(0.128)	(0.132)	(0.140)	(0.144)
Net ODA and aid per capita	0.000181	0.000630	-0.00250***	-0.00224**	0.000219	0.000113	-6.41e-05	0.000393	0.000312	0.000239	-0.000766	-0.000526
	(0.00155)	(0.00150)	(0.000931)	(0.000915)	(0.00155)	(0.00160)	(0.00171)	(0.00167)	(0.00208)	(0.00213)	(0.00159)	(0.00154)
Population density	0.000531	0.000557	0.000152	0.000167	-0.000229	-0.000235	-0.000169	-0.000138	0.000445	0.000440	5.69e-05	7.11e-05
	(0.000754)	(0.000716)	(0.000720)	(0.000667)	(0.000450)	(0.000459)	(0.000604)	(0.000552)	(0.000596)	(0.000592)	(0.000759)	(0.000780)
External war	0.283**	0.297**	0.177	0.185	0.0928	0.0894	0.178	0.193*	0.171	0.169	0.200**	0.208**
	(0.137)	(0.136)	(0.123)	(0.125)	(0.138)	(0.140)	(0.112)	(0.112)	(0.127)	(0.129)	(0.0986)	(0.0991)
Civil war	-0.0363	-0.0326	0.0328	0.0349	0.126	0.125	-0.182	-0.179	0.175	0.174	-0.220	-0.218
	(0.237)	(0.230)	(0.212)	(0.217)	(0.259)	(0.264)	(0.203)	(0.203)	(0.193)	(0.196)	(0.221)	(0.218)
statehist50v3	0.00439	0.00522	0.00440	0.00488	0.000380	0.000182	0.00626	0.00715	0.00563	0.00549	0.00458	0.00502
	(0.00517)	(0.00519)	(0.00412)	(0.00407)	(0.00269)	(0.00278)	(0.00550)	(0.00527)	(0.00370)	(0.00378)	(0.00345)	(0.00363)
Constant	0.589	0.923	-0.266	-0.0762	0.622	0.542	0.350	0.690	1.501*	1.447	2.266***	2.445***
	(0.834)	(0.864)	(0.745)	(0.769)	(0.805)	(0.826)	(0.936)	(1.001)	(0.881)	(0.912)	(0.777)	(0.810)
Observations	61	61	61	61	61	61	62	62	62	62	61	61
Adjusted R-squared	0.206	0.236	0.234	0.238	0.278	0.266	0.110	0.136	0.344	0.332	0.025	0.023
Joint(p)		0.0380		0.553		0.434		0.127		0.874		0.420

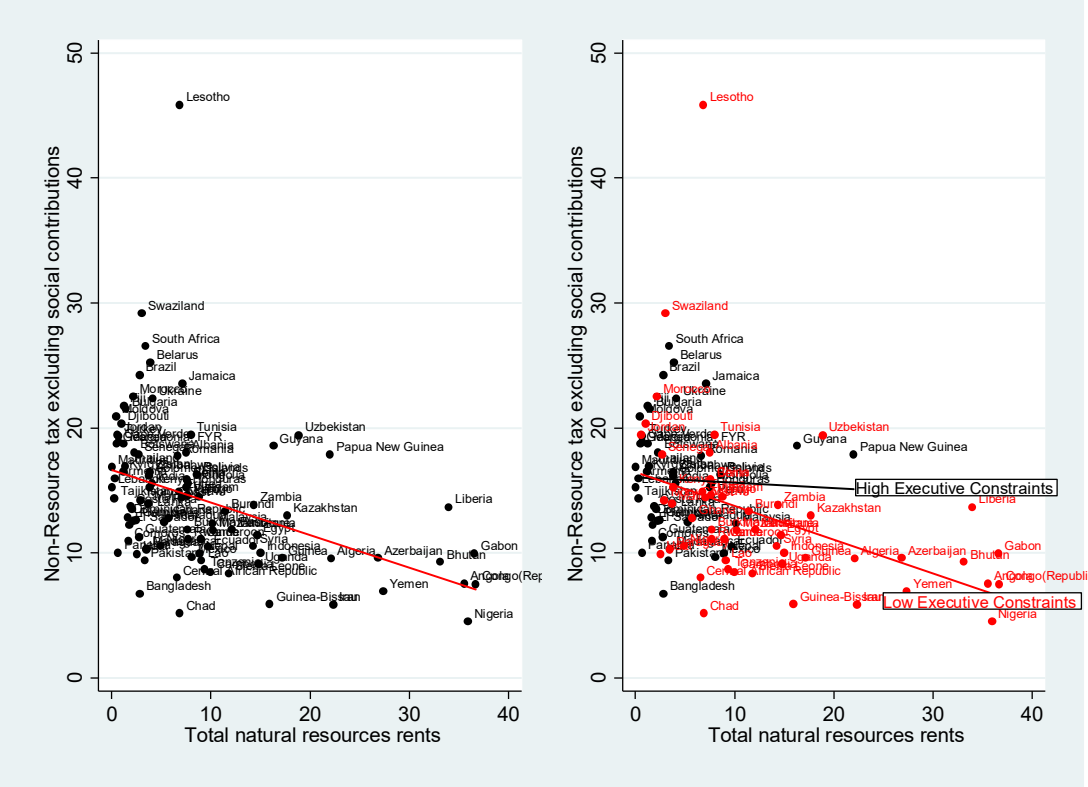
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.11 – Effects of resource intensity on fiscal capacity at different levels of executive constraints

executive constraints	Transparency of taxpayer obligations and liabilities b/se	Tax appeals mechanisms b/se	Controls in the taxpayer registration system b/se	Effectiveness of penalties for non-compliance with registration and tax declaration b/se	Quality of tax audit b/se	Effectiveness of transfer of tax collections to the Treasury by the revenue administration. b/se
1	-0.029** (0.01)	-0.016 (0.02)	-0.004 (0.01)	-0.032* (0.02)	0.002 (0.01)	-0.011 (0.02)
2	-0.017* (0.01)	-0.009 (0.01)	-0.007 (0.01)	-0.020 (0.02)	0.001 (0.01)	-0.005 (0.02)
3	-0.006 (0.01)	-0.003 (0.01)	-0.010 (0.01)	-0.007 (0.02)	-0.001 (0.01)	0.001 (0.01)
4	0.006 (0.01)	0.004 (0.01)	-0.013 (0.01)	0.005 (0.02)	-0.003 (0.01)	0.008 (0.01)
5	0.018* (0.01)	0.011 (0.02)	-0.015 (0.01)	0.017 (0.02)	-0.005 (0.01)	0.014 (0.01)
6	0.030** (0.01)	0.017 (0.02)	-0.018 (0.02)	0.029 (0.03)	-0.007 (0.02)	0.020 (0.02)
7	0.042** (0.02)	0.024 (0.03)	-0.021 (0.02)	0.041 (0.03)	-0.009 (0.02)	0.027 (0.02)

Notes: The marginal effects of total natural resources rents are calculated using the coefficients from Table 10, Columns 2, 4, 6, 8, 10, and 12

Figure 3.1 - Relationship between non resource-tax and natural resources rents



Appendix

Table 3.A.1 – Variables and sources

Variable	Description	Source
Non-Resource tax excluding social contributions	Non-resource component of total tax Revenue excluding social contributions and natural resource revenue	International Centre for Tax and Development
Fiscal capacity	Non-resource component of taxes on income, profits, and capital gains as a percentage of non-resource component of total tax revenue excluding social contributions and natural resource revenue	Own elaboration based on data from the International Centre for Tax and Development
Executive constraints	Institutionalised constraints on the decision making power of chief executives ranging from 1 (unlimited authority) to 7 (limited authority). Values outside [1;7] are treated as missing.	Polity IV Project, Center for Systemic Peace (Marshall et al. 2014)
Total natural resources rents	Total natural resources rents (% of GDP). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
Forest rents	Forest rents (% of GDP). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
Oil rents	Oil rents (% of GDP). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
Gas rents	Gas rents (% of GDP). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
Mineral rents	Mineral rents (% of GDP). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
Political stability	Sum of xropen (openness of executive recruitment) and xrcomp (competitiveness of executive recruitment) variables in the Polity IV dataset ranging from 2 (instable) to 7 (stable)	Polity IV Project, Center for Systemic Peace (Marshall et al. 2014)
External Debt	External debt stocks (% of GNI). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
Trade	Trade (% of GDP). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
Net ODA and aid per capita	Net official development assistance and official aid received (constant 2013 US$) per capita. Data are averaged over the period t-4 to t-1	Own elaboration based on data from World Bank – World development Indicators
Population density	Population density (people per sq. km of land area). Data are averaged over the period t-4 to t-1	World Bank – World development Indicators
External war	Hostility level of interstate dispute ranging from 0 (no dispute) to 5 (war). Data are averaged over the period t-4 to t-1	Palmer et al. (2015)
Civil war	Intensity level of Internal and internationalised internal armed conflict ranging from 0 (no conflict) to 2 (more than 1000 battle-related deaths). Data are averaged over the period t-4 to t-1	UCDP/PRIO Armed Conflict Dataset

Figure 3.A.1 - Relationship between non resource-tax and natural resources rents

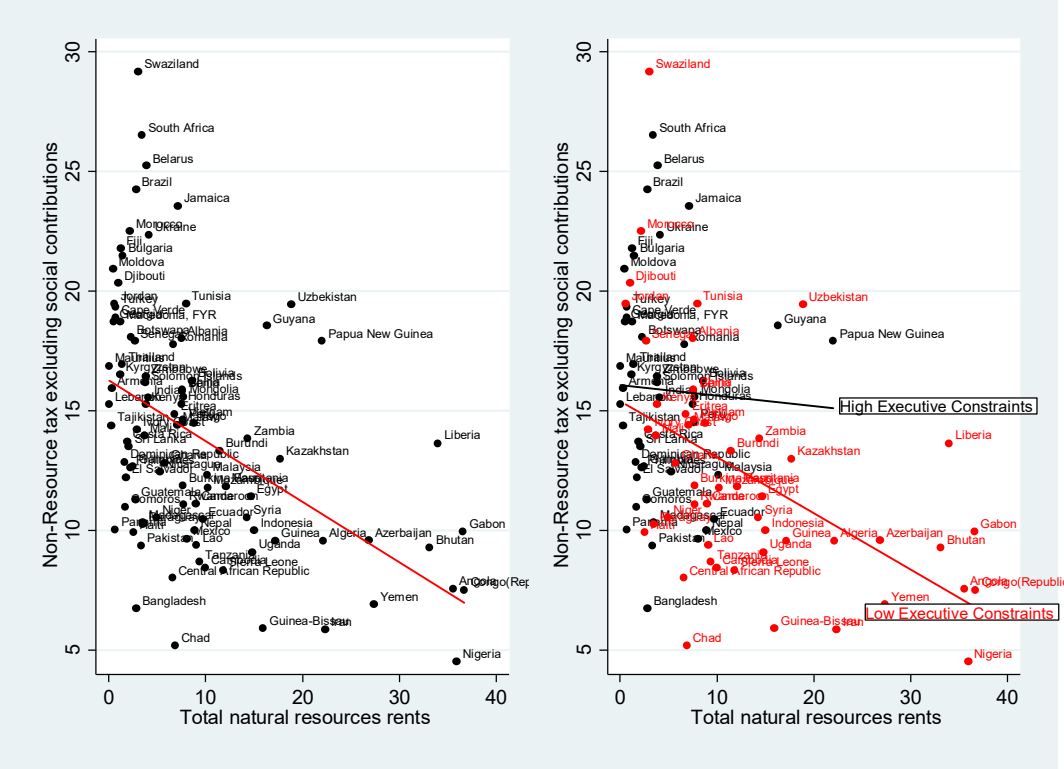


Figure 3.A.2 – Marginal effects of total natural resources rents at different levels of executive constraints

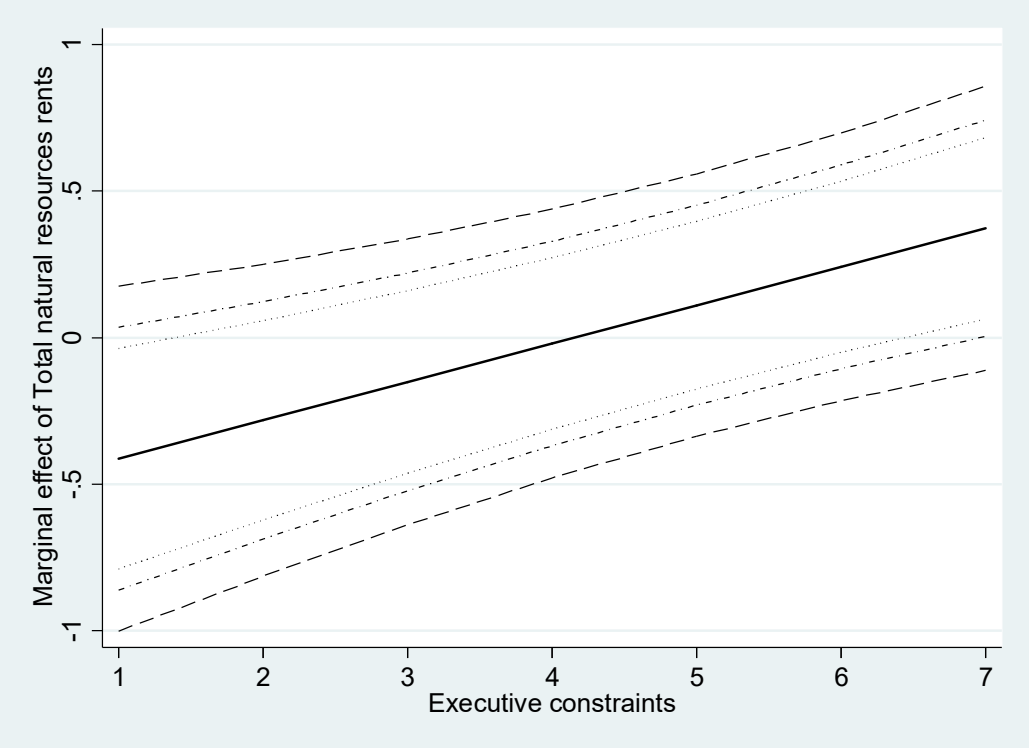


Figure 3.A.3 – Marginal effects of forest rents at different levels of executive constraints

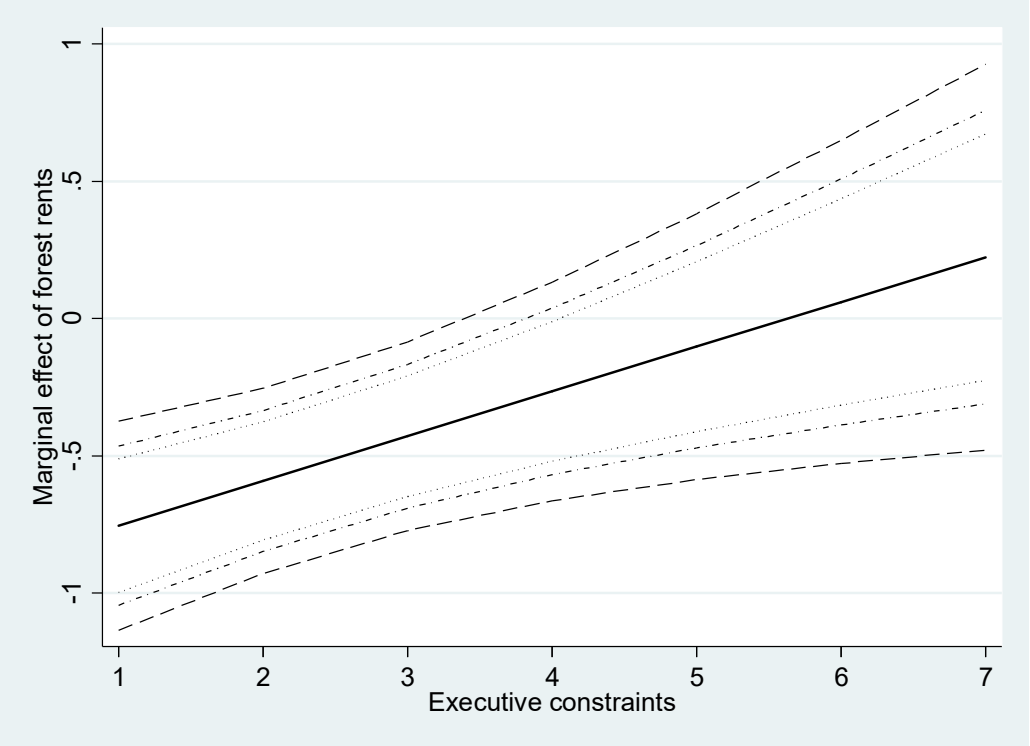


Figure 3.A.4 – Marginal effects of oil rents at different levels of executive constraints

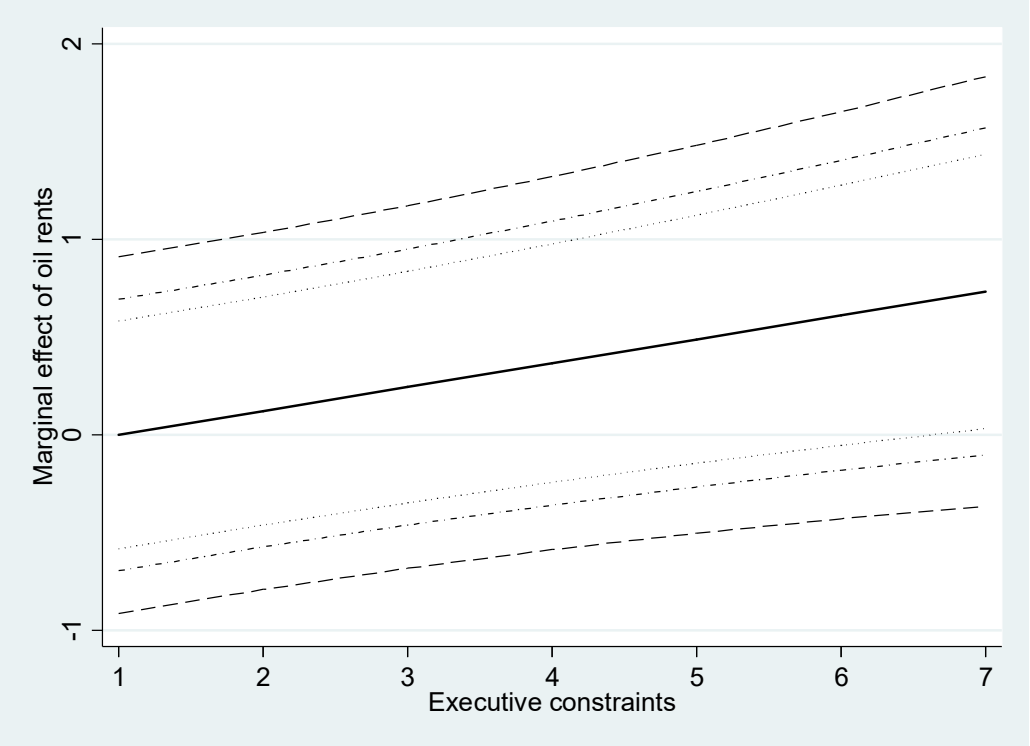


Figure 3.A.5 – Marginal effects of gas rents at different levels of executive constraints

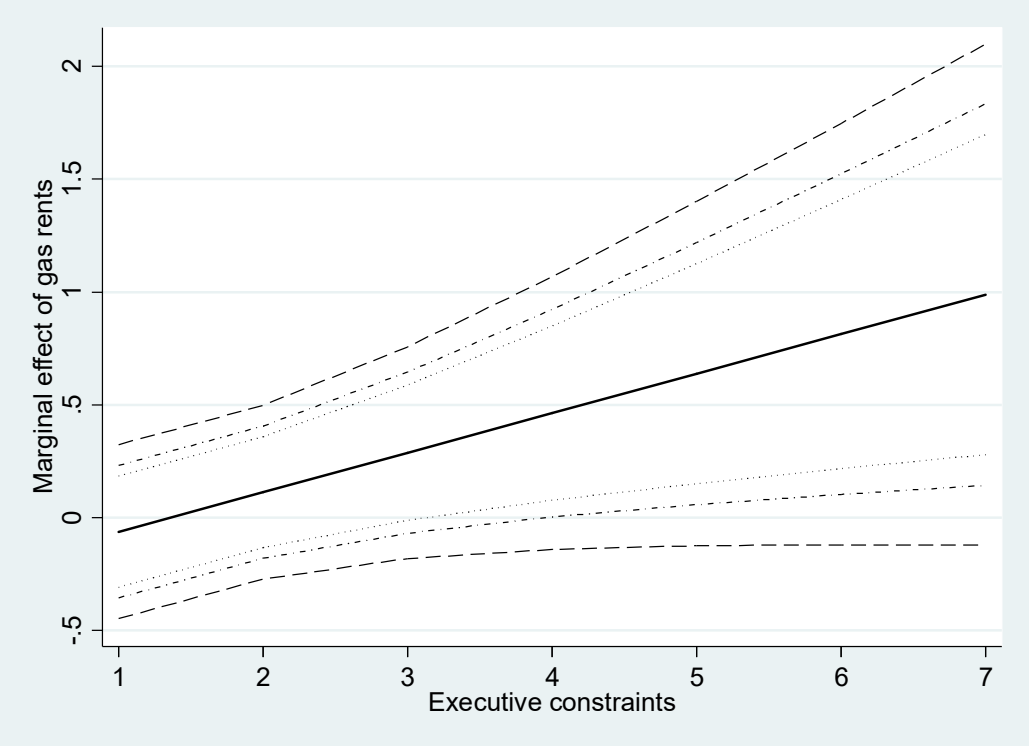
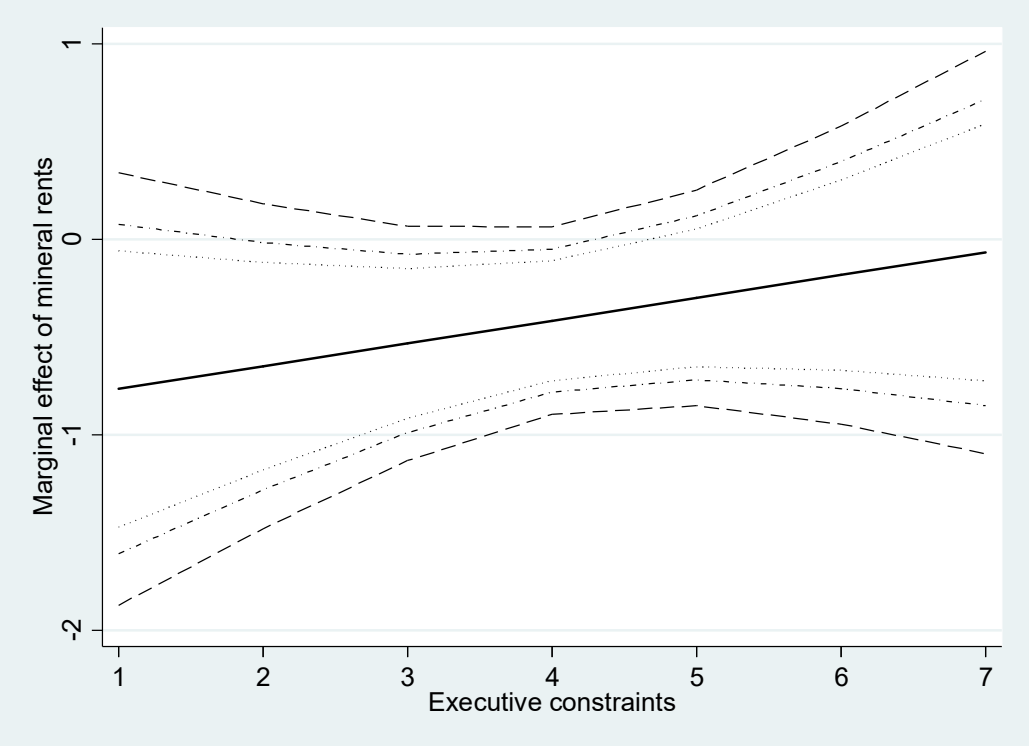


Figure 3.A.6 – Marginal effects of mineral rents at different levels of executive constraints



Conclusions

This thesis comprises three essays that investigate institutions, how they may be affected by internal and external factors, and their role in economic development. The first essay analyses the impact of civil society organizations and democracy assistance on political institutions. Using the UNDEF project database, which provides the conditions for treatment effect evaluations and allows a cross-country analysis to be carried out, the paper shows that the intervention of local civil society organizations supported by an international organization such as the United Nations may lead to increased democracy level in recipient countries but only if repeated over time. The essay confirms that projects with specific aims may be more effective in democratisation than large amounts of general economic aid, even over a short period of time (Scott and Steele, 2011), and are consistent with Persson and Tabellini (2009) who claim that democracy emerges through the accumulation of a stock of civic and social assets. It suggests that the beneficial effect of UNDEF projects could be explained by the ex-post evaluation that indirectly affects both the CSO involved in the project, and the institutions benefiting from its activity, or by the repeated interaction between UNDEF governance and recipient countries. However, further analysis should be carried out to understand the mechanisms through which aid and local civil society organizations affect political institutions.

The second and the third essays contribute to the literature on the resource curse, the counter-intuitive finding that countries highly endowed in exploitable natural resources perform worse than those in which this asset is lacking. To take into account the heterogeneous effect of natural resources across countries, the second essay carries out a case-study analysis and evaluates the effect of the variation in oil endowment on political regimes. Using the synthetic control method, it confirms the idea that natural resources may be a curse or a blessing for a country, depending on the quality of its institutions (Mehlum et al. 2006; Robinson et al. 2006). In particular, this essay shows that the relationship

between natural resources and democracy depends on the initial level of democracy itself. Indeed, only countries with a high level of democracy before the peak of oil discoveries are unaffected by it. A plausible explanation for these results is that, as the rate of discoveries starts to decline, incumbents enforce higher entry barriers to grab the residual resources. However, this is prevented by a high level of executive constraints.

Relying on the same hypothesis according to which institutions can mitigate or actually reverse the resource curse, the third essay investigates how natural resources affect the incentives for investing in fiscal capacity, and the role of political institutions in this process. The share of non-resource taxes on income, profits, and capital gains in total non-resource taxes is used as a proxy for fiscal capacity. This measure allows the ability to raise taxes to be distinguished from a government's policy choices, given that collecting income taxes compared to other types of taxes requires major investments in fiscal infrastructure (Besley and Persson, 2011). The panel analysis shows that resource rents are negatively associated with fiscal capacity, but countries with a high level of executive constraints are able to neutralise or reverse this effect, depending on the type of resource endowments. The essay seeks to identify the specific fiscal institutions affected by natural resources using a recently created set of indicators from the Public Expenditure and Financial Accountability project. The findings indicate that the effect of natural resources is likely to work through fiscal institutions that make the state accountable to, and transparent for, its citizens.

Overall, this thesis confirms that "institutions rule" (Rodrik et al., 2004). Indeed, institutional quality is essential to at least counterbalance the impact of exogenous factors such as natural resources on development outcomes. Moreover, it provides some reason for optimism about the possibility of taking action to improve these institutions.

References

Besley, T., Persson, T. (2011). *Pillars of prosperity: The political economics of development clusters*. Princeton University Press.

Mehlum, H., K. Moene, and R. Torvik (2006). Institutions and the resource curse. *Economic Journal*, 116(508): 1–20.

Persson, T., Tabellini, G. (2009). Democratic Capital: The Nexus of Political and Economic Change. *American Economic Journal: Macroeconomics, 1(2), 88-126.*

Robinson, J., R. Torvik, and T. Verdier (2006). Political foundations of the resource curse. *Journal of Development Economics*, 79(2): 447–68.

Rodrik, D., Subramanian, A., Trebbi, F. (2004). Institutions rule: the primacy of institutions over geography and integration in economic development. *Journal of economic growth*, *9*(2), 131-165.

Scott, J. M., Steele, C. A. (2011). Sponsoring Democracy: The United States and Democracy Aid to the Developing World, 1988–20011. *International Studies Quarterly, 55, 47-69.*